JOURNAL

OF

THE FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

AUGUST, 1862.

CIVIL ENGINEERING.

Description of the Centre Pier of the Bridge across the River Tamar at Saltash, on the Cornwall Railway, and of the means employed for its construction. By R. P. BRERETON, M. Inst. C. E.

From the Lond. Civ. Eng. and Arch. Journal, April, 1862.

THIS communication embraced, in a narrative form, a detailed account of the preliminaries connected with the Albert Bridge, which crossed the river Tamar where it was only 1100 feet wide, with precipitous banks and a depth of water to the surface of the mud of 70 feet. A dyke of greenstone trap intersected the clay slate formation at this point, and cropped out to the surface above the water on the western bank of the river. It was ascertained by borings made in the bed of the river that rock extended from the eastern side to beyond the middle of the stream, covered with mud or silt to a depth of from 3 feet to 16 feet. Subsequently, a thorough examination of the bed of the river where a centre pier would probably be built, by means of 175 borings made within a cylinder at thirty-five different places, over an area of 50 feet square, enabled an exact model of the surface of the rock to be prepared, showing the irregularities and fissures that might be expected. Eventually it was decided, from the information thus obtained, to erect one pier only in the deep water, instead of three, as would have been necessary for the spans required by the Admiralty; and when it was determined to proceed with the construction of the bridge, in 1852, it was decided that there should be two spans of 455 feet, two of 93 feet, two of 83 feet 6 inches, two of 78

feet, two of 72 feet 6 inches, and nine of 69 feet 6 inches; the total length, including the adjoining land openings, being 2200 feet.

The centre, or deep water pier, intended to carry the weight of one-half of each of the two main spans, consisted of a column, or circular pillar, of solid masonry, 35 feet diameter and 96 feet high, carried up from the rock foundation to above high-water mark. Upon this were placed four octagonal columns of cast iron, 10 feet diameter, carried up to the level of the roadway, which was 100 feet above high water mark. Upon the tops of the columns, cast iron standards were fixed to receive the ends of the tubes and chains which constituted the trusses of the bridge. The weight at the bottom of the masonry foundations was about $9\frac{1}{2}$ tons per square foot, increased, when the bridge was loaded by passing trains, to about 10 tons per square foot.

In the construction of the masonry pier, a wrought iron cylinder, of boiler plates, 37 feet in diameter and 90 feet in length, and open at the top and the bottom, was sunk through the mud of the bed of the river to the rock. The water was then pumped out, and the mud excavated, the masonry being built up inside, and the cylinder above the ground afterwards removed. It was expected that, by forming a bank round the cylinder after being sunk to the rock, sufficient water-tightness would be ensured for getting in the masonry. To provide, however, for the contingency of excessive leakage, the cylinder was so constructed as to admit of the application of air pressure. As the surface of the rock, although very irregular and ragged, had a general dip to the south-west, the bottom of the cylinder was formed with a corresponding bevel, one side being 6 feet longer than the other. A dome, or lower deck, was constructed inside, at the level of the mud, and an internal cylinder, 10 feet in diameter, open at the top and the bottom, connected the lower with the upper deck of the cylinder. The 6 feet cylinder, previously used for the borings, was fixed eccentrically inside the other, and an air-jacket or gallery, making an inner skin round the bottom edge below the dome, was formed, about 4 feet in width, divided into eleven compartments, and connected with the bottom of the 6 feet cylinder by an air passage below the dome.

Details were then given of the construction of the larger cylinder, and of the mode of launching and floating it to its position. When accurately adjusted over the intended site, water was gradually let in until the cylinder penetrated through the mud about 13 feet and rested on some irregularities upon the rock, which caused it to heel over towards the east about 7 feet 6 inches. By letting water in upon the dome or lower deck, and loading the higher side with iron ballast, the cylinder forced its way through the obstructions at the bottom edge, and took a nearly vertical position. The air and water pumps were then set to work, and the greater part of the mud and oyster shells, which filled the compartments of the air-jacket at the bottom, was cleared out, and the irregular surface of the rock excavated: the bottom of the cylinder being now 82 feet below high-water. Subsequently, a leak having broken out through a fissure in the rock on the north east, or higher edge, considerable difficulty was experienced in main-

taining sufficient pressure with the air-pumps to keep the water down and the bottom dry. The leak was at length reduced, by driving close sheet piling into the fissure. When at its full depth, the cylinder was 87 feet 6 inches below high-water at the lowest place, and then a hemp gasket was worked under the edge of the cylinder, all round the outside, to assist its water-tightness. A ring of granite ashlar, 4 feet in width and about 7 feet in height, was then built in the air-jacket; and a bank of clay and sand was deposited round the outside of the cylinder to compress the mud. When the water was pumped out of the cylinder below the dome, and the excavation of the mud was being proceeded with, a leak broke out, and the water overpowered the pumps. Additional engines and pumps were provided, and efforts were made to diminish the leakage, with varying success; but as it required four pumps to keep the water down to 54 feet, recourse to air pressure in the body of the cylinder below the dome became imminent, and preparations for its application were made. To provide against the buoyancy, or upward pressure against dome and cover, the 37 feet cylinder was loaded with 750 tons of ballast, in addition to its own weight of 290 tons. The pumps were then got in good order, and by continued pumping, succeeded in keeping the water down. The mud was excavated, the cylinder below the dome securely shored across, and the rock levelled, when the masonry in thin courses of granite ashlar in cement, in the body of the cylinder was commenced. As soon as the masonry reached the level of the air-jacket ring, it was thoroughly bonded, the plates of the air-jacket being cut out as it proceeded. Upon the top of the bonding course, two courses of hard brick work in cement were laid, making a perfectly water-tight floor over the whole diameter of the column. Meanwhile, the masonry of the air-jacket, where the leak occurred, was taken down, and the leak was diminished by additional sheet-piling. The leak was discovered to have broken out at the same fissure as before, and had torn away the rock underneath the masonry of the air-jacket and bottom edge of the cylinder, but the masonry itself was undisturbed.

The next operation was to draw off the water above the dome and remove the ballast, to allow the masonry to be proceeded with, which it eventually did at the rate of from 5 to 7 feet in height per week. When it was 46 feet in height the influx of water was entirely stopped. After the masonry had been completed to the level of the plinth, the upper part of the cylinder was unbolted at the separate joints, and floated to the shore.

Proceedings Instl. Civil Engineers.

New Railroad System of M. L. D. GIRARD.—*Sliding Cars.*

This new method, which is said to have attracted the attention and excited the astonishment of the Emperor, so that he condescended to take a ride on the model road 40 yards long; and which is of course occupying the attention of all the journals, appears to consist in this: The cars are supported upon hollow sleigh-runners, from which water

is forced under a pressure sufficient to lift the weight almost entirely off the rails: the escaping water moreover lubricating the rail. A water-turbine is the mode of propulsion suggested, but it is not precisely stated that this was the mode used in the *immortal* experiment. M. Girard says that his last experiments gave a friction of 4·3 kil. (9·48 lbs.) per ton (about $\frac{1}{233}$); while, so soon as the water was shut off the friction rose to 550 kil. (1102·37 lbs.) (about $\frac{1}{2}$). Now, according to Gen. Morin's experiments, the friction with water as a lubric is about $\frac{1}{4}$. It appears, therefore, that the hydraulic pressure was sufficient to lift $\frac{5}{6}\frac{2}{0}$ths of the weight. Make the calculation for an ordinary railroad train, freight or passengers, and see what is the practical value of this "*most important discovery.*"

Description of the Loch Ken Viaduct, Portpatrick Railway.

By E. L. J. Blyth, M. Inst. C. E.

From the Civ. Eng. and Arch. Jour., April, 1862.

This viaduct was situated on a curve of half a mile radius, and carried a single line of railway over the loch at an oblique angle, so that the width of the water-way was increased from 265 feet to 360 feet, the depth of the water at the point of crossing being 29 feet in summer. It consisted of seven openings—three of 130 feet each in the centre, spanned by wrought iron girders of the bow-and-string form; two semicircular arches of masonry, of 20 feet span, in the abutments; and two openings of 20 feet each at the ends, provided with flat cast iron girders. Owing to there being scarcely any current, it was not deemed necessary to set the piers in the line of the loch, but they were placed at right angles to the viaduct, and each pair of girders was at a slight angle to the adjacent ones.

The foundations consisted of strong gravel, except in the case of the east abutment of the main openings, where a running sand was met with, and in this instance the lower courses of the masonry were laid on a bed of hydraulic lime concrete 2 feet in thickness. The two deep water piers were each formed of two towers, 8 feet in diameter, placed 8 feet apart, and connected above the water level by semicircular arches of masonry. For each tower of the piers a cast iron tube 8 feet in diameter, in six pieces, was sunk, the tubes being 36 feet and 42 feet in length for the east and west piers respectively. When the masonry was brought up to the surface, the upper castings of the tubes were removed. Around the piers 4000 cubic yards of loose rubble stones were deposited, so as to produce an artificially deeper foundation. The tubes when placed in position, sank from 1 foot to 2 feet by their own weight, until they reached the gravel and sand, where they remained quite firm. This formed a good test of the sufficiency of the foundation, as the weight of the tubes on their narrow edges was equal to from 8 to 9½ tons per square foot, while the total weight on the foundations of the finished structure, including the moving load, was only about 6¼ tons per square foot.

The method adopted in sinking the tubes was that of ordinary well-

sinking. Two plate-iron screw pans, of an inverted cone shape, were employed; one 2 feet in diameter at the top and 1 foot deep, and the other, which was only used for the harder portions of the excavation, 1 foot in diameter at the top and 1 foot deep. There were openings in the sides, covered with leather flaps, to prevent the material from escaping when the pans were filled. Three arms of round iron projected through the sides of the pans, and, being connected to a long rod with a cross handle at the upper end, the screw pans were worked by four men, and when full were raised by tackle. The larger pan raised about 1 cubic foot of material each time, and the smaller one about one-fourth of that quantity. By these means the tubes were sunk in some instances as much as 18 inches in one day, the minimum being 2 inches per day in the case of the north tube of the west pier, where large boulder stones were encountered, rendering necessary the use of a screw pick. When the tubes had been lowered the desired depth, concrete was deposited within them, varying from 12 feet to 18 feet in depth in each tube. On this concrete, ashlar masonry was laid, the cordon course being of granite in large blocks, for receiving the ends of the girders, which rested on wrought iron plates, laid on thick sheets of vulcanized india-rubber to lessen the effect of vibration.

The bow-and-string girders were each 136 ft. 8 ins. in length, and were segmental in form, the rise being 17 ft. 6 ins., so that the segment was almost identical with a catenary curve, or the true curve of equal pressure. The sections of the upper and the under booms were identical. They consisted of a main plate, 24 ins. broad and $\frac{3}{4}$ of an inch thick, and of two channel irons, each 8 ins. by 4 ins. in section and $\frac{1}{2}$ an inch thick, placed at a distance of 8 inches apart, between and to which the struts and ties, of the same section of channel iron, were riveted. The transverse girders for carrying the roadway were 6 inches in depth at the ends where they rested on the channel irons of the under booms, and 15 inches deep in the centre. The middle web of these girders was $\frac{1}{4}$ of an inch in thickness, and there were angle-irons, 3 inches by 3 inches by $\frac{1}{2}$ an inch in section, at the top and the bottom of the web on each side. Every alternate girder projected 2 feet, from which T iron struts were carried up to the crossings of the diagonal bracing. The weight of the girders and roadway between the points of support was 88 tons, and of the ballast (2 inches in depth) 14 tons, making a total dead load of 102 tons; and taking the rolling load at 1 ton per lineal foot, the total load on one span would be 232 tons. The area of the upper boom was 33 inches, and of the under boom, exclusive of rivets, 27·04 inches. The distance between the centres of gravity of the upper and the under booms was 17·04 inches. The tensile strain on the under boom amounted to 4·04 tons per inch, and the compressive strain on the upper boom to 3·35 tons per inch. When the whole of the load was upon the girders there was no compressive strain on any of the diagonals, but there were tensile strains varying from 3·4 tons to 7·5 tons, or equal respectively to 9 cwts. and 1 ton per square inch of section.

The author considered that the bow-and-string girder possessed ad-

vantages over the Warren or other lattice girders, with parallel top and bottom members; as in the latter class it was not possible to make the top and bottom members theoretically correct, without great labor and waste of material, and as owing to the great variation in the strains on the diagonals, it was necessary that they should be of varying dimensions, involving in some cases even different sections of iron.

The girders were built in position on staging, and the greatest amount of deflection of any one girder from its own weight was $\frac{3}{8}$ths of an inch. Subsequently, when a locomotive engine, weighing 34 tons, was placed in the centre of each span, and afterwards was run over, first at 10 miles an hour, and then at 25 miles, the deflection amounted to from $\frac{3}{16}$ to $\frac{1}{4}$ of an inch in each girder, there being no perceptible difference in either case. Finally when four engines were coupled together, so as to give a load equal to 1 ton per lineal foot, the deflection only amounted to from $\frac{1}{2}$ to $\frac{5}{8}$ths of an inch.

It was stated, that the total cost of this viaduct had amounted to about £13,000.

Proc. Civ. Eng. Soc.

On Railway Accidents. By Capt. Douglas Galton, R. E., F. R. S., Assoc. Inst. C. E.

From Newton's London Journal, May, 1862.

(Continued from p. 21.)

It was stated that the length of railway communication open in the British Isles at the end of 1860, was 10,433 miles, upon which 163,435,678 passengers were conveyed in that year. From official returns it appeared that, during the seven years ending the 31st of December, 1860, there were 116 passengers killed, and 2832 injured, from causes beyond their own control. From the sums paid by railway companies for compensation, it was calculated that an insurance of one twenty-fourth part of a farthing per passenger per mile would, on the average of all lines, cover the cost of railway accidents. It had been found impossible to obtain reliable information as to the number of coach accidents in this country; but the returns of the Messageries Impériales showed that, in a series of years, the number of passengers killed and injured, from causes beyond their own control, was 1 in 28,000. From the latest comparative returns, the number of passengers killed and injured was, on British railways, 1 in 334,000; on Belgian railways, 1 in 1,600,000; on Prussian railways, 1 in 3,000,000; and on French railways, 1 in 4,000,000. The greater comparative safety of foreign railways was traced to differences in the conditions of the traffic and of the management, as well as in the habits of the people.

In endeavoring to elucidate the question, whether any of the accidents which had occurred could have been prevented by reasonable precautions, the first point which arose was, the extent to which the amount of traffic on the several lines influenced the number of accidents. The general averages thus obtained showed that lines of small traffic were comparatively safe; but as traffic alone did not determine

the number of accidents, it was necessary to analyze the causes in detail—taking, first, those which could not be guarded against; and, secondly, those which were within the control of the managing or working staff. During the seven years before referred to, 534 accidents to trains had been reported upon by the inspecting officers of the Board of Trade, in which 2912 passengers were killed or injured. In many of these cases there had been more than one contributing cause, but the majority might be thus tabulated:—

	Number of Accidents reported upon.	Number of Sufferers.	Cases in which the Accidents could not be guarded against.	Cases in which the Accidents were due to Causes within the Control of the Management.		
				Attributable to the Works or Rolling Stock.	Attributable to the System of Working.	Negligence of Inferior Servants.
Accidents from Engines and Carriages leaving the Rails, or Fractures of Machinery,	135	313	59	98	15	17
Collisions of every description,	319	2532	16	222	219	183

These figures showed that a large proportion of the so-called accidents were due to preventible causes. Those arising from the fracture of axles and tyres, and from engines and carriages leaving the rails, were less than one-half of the number which could not have been guarded against. But out of the 319 collisions, only 16 were attributable to purely accidental causes, whilst 183 were assigned to the negligence of inferior servants, and 120 to the manner in which the traffic was conducted, and which ought not, therefore, to have occurred.

With regard to the first class of cases—accidents which could not have been guarded against—the author remarked, that the best form of tyre for a railway wheel had not yet been definitely settled. The wheels and axles could scarcely be said to be mechanically satisfactory; the form of brake in use was also imperfect. Although simple negligence could not be entirely prevented, yet in several cases the negligence had been attributable to the defective arrangement of the company, in permitting pointsmen and engine-drivers to be habitually over-worked. Those accidents which arose from trains passing on to a wrong line through facing points might not have occurred, if an indicator had been attached to the points, to show in which direction they were set. The comparatively small number of accidents from negligence alone, afforded strong evidence of the efficacy of the direct responsibility of the inferior servants. A few instances were then cursorily alluded to, in illustration of those accidents which were wholly or partially attributed to defects in the condition of the railway or the vehicles, or to the absence of the requisite auxiliaries to safety, such

as signals, brakes, &c. It was observed, that it was not for want of good rules that accidents occurred, but for want of a continued enforcement of those rules, and a close examination into the details of the manner in which the traffic was worked.

The discussions which had taken place on this subject in Parliament, both in 1853, and again in 1857, were then considered, and the conclusion was arrived at, that freedom from railway accidents was not to be obtained by Government interference, but by an effective and responsible internal management, which would enforce the greatest punctuality and care in working the traffic, and maintain the strictest discipline amongst the servants employed.

The existing law affecting railway companies as carriers was then alluded to; and attention was next called to the principle of compensation for injuries sustained,—Lord Campbell's Act being specially cited as the Parliamentary recognition of that principle. It was said that this Act removed a technical difficulty in the way of recovering compensation, rather than gave a new right to compensation. The money payment thus provided operated as a punishment, and tended to prevent the commission of careless acts. Compensation might, therefore, be looked upon partly as a penalty upon the company for its corporate carelessness, and partly as a remedy to the sufferer for the injury received. If viewed as a remedy, it should be such as to tend to prevent a recurrence of the act for which punishment was awarded. It should, therefore, depend on the degree of blame which attached to the management for the accident, and it should be equally certain and just in its operation. In its aspect as a remedy, it should be easily recoverable by the sufferer. As at present levied, it did not properly fulfil either of these conditions, for reasons which were stated. Assuming that such a maximum amount was fixed upon as would fairly compensate the generality of passengers, according to the class in which they were traveling, and assuming that it were made payable in the case of every accident which occurred, beyond the control of the passengers, without there being any obligation to prove negligence, the author was inclined to think that the fine would be rendered more certain in its operation, but that as a preventive, the effect of the alteration would not be appreciable. The true remedy against railway accidents lay, in the author's opinion, with the railway companies themselves. Improved management would be greatly assisted by placing at the head of each railway a director of adequate capacity, responsible to the board for the management of the concern, who should be required to devote the whole of his time to its interests, and be paid in proportion; by giving the chief officers of the railway control of, and making them responsible for, the several departments, so that they might be held answerable for the results; and by providing a gradation of responsibility throughout all the employés. Improvements in the machinery and system of working might be promoted by the formation of an association amongst railway companies, embracing the objects of the association between the German railway companies, and of the association between manufacturers, near Man-

chester, for the prevention of boiler explosions. It was doubtful, however, whether such an association could become of any practical utility in this country, unless it assumed the form of an association for the purpose of mutual insurance against accidents managed by a board of railway officials, chosen from the associated companies.

Proc. Inst. of Civil Engineers, April 1st, 1862.

The Economic Angles in Parallel Open-work Girders.

From the Lond. Civ. Eng. and Arch. Journal, May, 1862.

(Continued from page 27*.)

We have hitherto spoken of each pair of braces as having to convey a certain vertical component = V, towards one only of the piers, say to pier Q, so that the first individual of the pair is always a strut or tie, while the other is always a tie or a strut. But there are some cases in which a pair of braces may be required to transmit a vertical component at one time in the direction of pier Q, and at another (from changes in the distribution of the loading) in the opposite direction, or towards pier P. This occurs in the central pairs of long girders when a large portion of the loading is movable, and still more strikingly in the case of the longitudinal stiffening girders applied to suspension bridges, the duty of which is to distribute as uniformly as may be over the span, the effect of a concentrated or irregular loading.

When the amount of action of the pair, or the value of V, is as great when directed towards one pier as when towards the other, the economic angles will evidently be each equal to 45°, whatever the ratio of a_1 to a_2. And many of the pairs in the stiffening girders of a suspension bridge will approximate more or less nearly to this condition. On the other hand, when the secondary value of V is less than $\frac{a_1}{a_2}$ times the first value (supposing ties to be the most economical braces, and $\frac{a_2}{a_1}$ times when struts are the most economical) then the secondary action may be neglected, since the angles θ_1 and θ_2 being calculated with reference to the greater value of V, there will nevertheless be sufficient material in the parts to carry the less value of V in the opposite direction; care must however be taken to give a suitable form of section to that brace which is in the first instance a tie, to fit it for acting as a strut under the less value of V.

The Application to Actual Structures.—The general conclusion arrived at in a previous paper, that, when one uniform value of a is adopted for both booms, the sum of all the numbers representing the weights of every bay of both booms, is unaltered by changes in the inclinations of the braces—so long as the distribution of the loading remains the same—is practically applicable, with an approximation to accuracy, to very few structures. The most important are the lon-

*The equivalent of x (as given in equations 5, on page 25,) should be multiplied by c.

gitudinal stiffening girders of suspension bridges, whose booms act alternately as struts and ties. There are some other structures that may be mentioned, the loadings or pressures on which may at different times act in opposite directions, but to which the investigation is scarcely applicable; such are the piers of viaducts, the longitudinal strengthening girders in some ships, horizontal girders to resist the wind, &c.

When a for the upper boom is taken greater or less than a for the lower one, the above conclusion would no longer hold good. And we might, therefore, very properly go on to investigate the economic angles for various girders having one uniform value of a for the top boom, and another uniform value of a for the bottom boom; this would lead us somewhat nearer to the truth, particularly in the case of structures having long spans in proportion to their depths. We will not, however, at least at present, enter upon this part of the subject, but proceed at once to the practical consideration of the general and much more interesting questions connected with the construction of complete girders in an economical manner.

The Economic Construction of Girders.

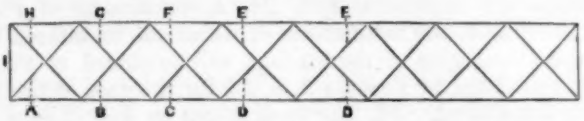
As the above title is a very comprehensive one, and, indeed, includes the subject we have just left, it is necessary to state that we do not intend at present to treat on the general subject of the economic construction of girders, but only employ the heading as an appropriate one under which to classify what follows in this, and also, it may be, in some future papers.

We purpose discussing certain girders in a thoroughly practical manner, taking no uniform value of the factor a for a boom, or for the struts of the bracing, or other parts, but adopting such a value of it for each particular bay of the booms, and for each particular brace, as shall be derived from executed structures, or dictated by constructive as well as theoretical considerations. It must be apparent that such a treatment of the subject removes it at once from the grasp of the higher forms of mathematical analysis; and our course of procedure will therefore be to tabulate the results of the separate calculations, based upon actual working data, of all those forms or varieties of any given structure which have any chance of being adopted; so that their respective merits and peculiarities may become obvious at a glance. Of course such a mode of investigation is both tedious and cumbrous, but it will be appreciated by a much wider circle of readers, and have a more direct bearing upon practical construction than any other mode; and we think it right to give the calculations in considerable detail, since the values of the factor a are taken somewhat arbitrarily, and as the forms of girder that will be made the subjects of discussion are such as will probably be employed in bridge construction to a very great extent indeed.

So long as the question of the economic angles is confined to cases in which the depth is less than or only one-sixteenth of the span (like the Newark Dyke Bridge, and many others which follow in this re-

spect the proportions of the Britannia Tube) as well as to suspension bridge stiffening girders, it is of very considerable importance; and the economic angles for the whole structure will approximate pretty

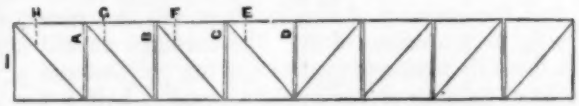
Fig. 1.



nearly to the values given in Tables I. or II. (pages 26 and 27). But when we accept the option which open-work construction offers us, of making the proportional depth twice as great as that of the Britannia Tube, with the highly economic advantage of a diminution of the stresses in the top and bottom of the girder by more than one-half, we shall find that the effect of variations in the value of θ, is very much complicated with and overborne by the influence of other elements that enter into the question.

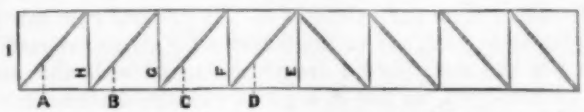
This point of depth of structure in proportion to span, is that on which must chiefly rest the comparison of plate with open-work girders; yet in a discussion, which extended over two evenings, at the

Fig. 2.



Institution of Civil Engineers, London, on the relative merits of these different modes of construction, it was altogether overlooked. In the letter already referred to (vol. xviii. p. 236*), the writer had the honor of being the first to draw attention to the matter, and further of advising the adoption for open-work girders of a depth equal to at least one-eighth of the span; and to this proportion he has very uniformly adhered in his practice as a consulting engineer, with the most gratifying economical results. Fig. 1 shows the girder of a viaduct constructed according to his drawings and calculations, two girders being employed to support two lines of railway; these girders have a depth equal to one-eighth of the span; and two series of braces, all placed at an angle of 45°, so that the span is divided into eight equal bays

Fig. 3.



or sub-spans. We purpose now giving the calculations of the quantities of material required for the construction of all the marked varieties of such a girder, under various conditions of loading and attachment to the piers, so that by tabling the results, their respective costs

*Civil Engineer and Architects' Journal.

can at once be ascertained, and hence the most economical form for the purpose in view made choice of.

The varieties of the pattern of the girder itself which we shall consider are *four*, viz:—1st. The regular form shown by Fig. 1, in which all the braces are inclined at 45°. 2d. A modification of Fig. 1, in which all the braces, the inclinations of which can be altered without affecting the distribution of the loading, will be placed at the economic angles calculated by formula 2, p. 24, for the case of the bracing alone being considered. 3d. The form shown by Fig. 2, in which all the struts are placed perpendicularly, and the ties at 45°, the number of series being reduced to one. And, 4th. The analogous form shown by Fig. 3, in which all the ties of the bracing are vertical, and the struts at 45°. The loading will be considered as applied in *two* different ways; 1st, as concentrated at the level of the upper boom; and, 2d, as wholly at the level of the lower boom. The modes of attaching the girder to the piers will be *three*, viz:—1st. By the extremities of both booms, so that no end pillar is required. 2d. By suspension, the attachment being at the extremities of the upper boom, and therefore the end piece acting as a tie. And, 3d, by direct support at the extremities of the lower boom, the end pillar being then a strut. There will therefore be results for twenty-four different arrangements or combinations, in all of which the span S, the depth D, the bays of the roadway, and the amount of the loadings, are the same; the only changes being in the values of θ. The loadings we will suppose to consist of a movable loading equal to one ton per foot run of the girder, and a fixed loading or dead weight equal to half a ton per foot run. Were other values of the dead weight assumed, the number of combinations would be so many-fold increased, but the estimate we have adopted is pretty well suited for ordinary railway bridges of certain spans.

The values of a employed in the calculations are, as far as found suitable, drawn from the completed viaduct already referred to, the whole being of wrought iron. The values of a are taken so great as to make a liberal allowance for joint-plates, for excess of material rendered advisable or unavoidable; for excess of length of parts above the calculated length between the points of intersection of the central lines; for rivets in the case of struts, required to build them up; and for loss of strength in the case of ties, from rivet holes, supposing these to be judiciously placed; these extras have been taken so liberally, that in some structures as much as 10 to 15 per cent. might be safely abstracted from the values of a. No general rule can be given for the value to be assigned to the factor a for struts: regard must be chiefly had to the unsupported length compared with the amount of stress to be conveyed, as this in a great measure determines the proportions of length to least width. Again, in girders, the form of section of the bracing struts will very properly be chosen with respect to the first or strongest, marked A in the figures; and as all the struts must, for constructive reasons, be of somewhat the same pattern, the shape determined upon will necessarily be very ill-suited for the central struts, and consequently the values of a for these should on this

account, as well as from their diminished stresses in proportion to their lengths, be greatly increased.

The depth D will be assumed as the unit of length, and one-twenty-fourth of the whole load on the girder will be taken as the unit of weight or stress; one-half only of the length of each girder will be calculated, and the weight of each part made = stress × length × a = v sec.$^2\theta$ a.

The details of the calculations for the case of the girders being of wrought iron are so fully given, that little trouble will attend the calculation of results for other materials or circumstances, since the requisite substitution of the appropriate values of a are rendered by such detail as clear as possible.

Calculations of the Weight of Iron in the Bracings and End Pillars.—1. In table I. are given the elements and results of the calculations for the first or regular form, fig. 1, when the roadway is situated at the level of the upper boom.

TABLE I.

Brace.	a.	v.	Sec.$^2\theta$.	Weight.
A	0·00060	6	2	0·00720
B	0·00067	4·5	2	0·00603
C	0·00077	3·25	2	0·00501
D	0·00090	2	2	0·00360
E	0·00120	1	2	0·00240
F	0·00045	2	2	0 00180
G	0·00045	3·25	2	0·00292
H	0·00045	4·5	2	0·00405
				0·03301
I as a tie,	0·00045	6	1	0·00270
I as a strut,	0·00060	6	1	0·00360

Table II. is for the girder as above, but with the roadway at the level of the lower boom.

TABLE II.

Brace.	a.	v.	Sec.$^2\theta$.	Weight.
A	0·00065	4 50	2	0·00585
B	0·00076	3·25	2	0·00494
C	0·00090	2·00	2	0·00360
D	0·00120	1·00	2	0·00240
E	0·00045	2·00	2	0·00180
F	0·00045	3·25	2	0·00292
G	0·00045	4·50	2	0·00405
H	0 00045	6·00	2	0·00540
				0·03096
I as a tie,	0·00045	6·00	1	0·00270
I as a strut,	0·00060	6·00	1	0·00360

In the first of the above tables, the values of a are taken in excess of, but as nearly as possible in proportion to, those for the actual viaduct. It will be observed that strut A in the second table has the same value of v as strut B in the first; but a better form of section may be chosen for the former, a, for it is therefore taken somewhat less than for B: the other values of a are assigned from similar considerations.

2. The elements and results of the calculations for the second form, or that with the economic angles as already explained, are given in tables III. and IV.; the first being for the case of the loading being at the level of the upper boom, the second for the loading concentrated at the lower level.

TABLE III.

Brace.	a.	θ.	v.	$\sec.^2\theta$.	Weight.
A	0·00060	45° 0′	6·00	2·0000	0·00720
B	0·00067	38 47	4·50	1·6457	0·00496
C	0·00077	36 25	3·25	1·5442	0·00386
D	0 00090	33 41	2·00	1·4442	0·00260
E	0·00120	45 0	1·00	2·0000	0·00240
F	0·00045	53 8	2·00	2·7782	0·00250
G	0·00045	51 37	3·25	2·5937	0·00379
H	0·00045	50 7	4·50	2·4321	0·00493
					0·03224
I as a tie,	0·00045	0	6·00	1·0000	0 00270
I as a strut,	0·00060	0	6·00	1·0000	0·00360

TABLE IV.

Brace.	a.	θ.	v.	$\sec.^2\theta$.	Weight.
A	0·00065	39° 17′	4·50	1·6691	0·00488
B	0·00076	36 39	3·25	1·5536	0·00384
C	0·00090	33 41	2·00	1·4442	0 00260
D	0 00120	45 0	1·00	2·0000	0·00240
E	0·00045	53 8	2·00	2·7782	0 00250
F	0·00045	51 29	3·25	2·5786	0·00377
G	0·00045	49 46	4·50	2·3970	0·00485
H	0·00045	45 0	6·00	2·0000	0·00540
					0 03024
I as a tie,	0·00045	0	6·00	1·0000	0·00270
I as a strut,	0·00060	0	6 00	1·0000	0·00360

3. Tables V. and VI. are for the third form of the girder, or that in which the struts of the bracing are vertical, and the ties at an inclination of 45° (fig. 2). The first of these tables is for the case of the load being at the upper, the second for the load being at the lower level.

TABLE V.

Brace.	a.	v.	Sec.$^2\theta$.	Weight.
A	0·00060	10·50	1	0·00630
B	0·00067	7·75	1	0·00519
C	0·00077	5·25	1	0·00404
½ of D	0·00090	1·50	1	0·00135
E	0·00045	3·00	2	0·00270
F	0·00045	5 25	2	0·00473
G	0·00045	7·75	2	0·00697
H	0·00045	10·50	2	0·00945
				0·04073
I as a tie,	∞	0·00	1	0·00075
I as a strut,	0 00060	12·00	1	0·00720

TABLE VI.

Brace.	a.	v.	Sec$^2\theta$.	Weight.
A	0·00065	7·75	1	0·00504
B	0·00076	5·25	1	0 00399
C	0·00090	3·00	1	0·00270
½ of D	∞	0·00	1	0·00075
E	0·00045	3·00	2	0·00270
F	0 00045	5·25	2	0·00472
G	0·00045	7·75	2	0·00698
H	0·00045	10·50	2	0·00945
				0·03633
I as a tie,	0·00060	1·50	1	0·00030
I as a strut,	0·00060	10·50	1	0·00690

4. The calculations of the weights of the various parts of the fourth form, fig. 3, having vertical bracing ties, the struts being inclined at angles of 45° therewith, are given for the two cases of the load being concentrated at the top and the bottom booms, in tables VII. and VIII., respectively.

TABLE VII.

Brace.	a.	v.	Sec.$^2\theta$.	Weight.
A	·00060	10·50	2	·01260
B	·00067	7 75	2	·01038
C	·00077	5·25	2	·00809
D	·00090	3·00	2	·00540
½ of E	∞	0·00	1	·00030
F	·00045	3·00	1	·00135
G	·00045	5·25	1	·00236
H	·00045	7·75	1	·00349
				·04397
I as a tie,	·00045	10·50	1	·00473
I as a strut,	·00120	1·50	1	·00180

TABLE VIII.

Brace.	*a.*	v.	Sec.^{2} *g.*	Weight.
A	·00060	10·50	2	·01260
B	·00067	7·75	2	·01030
C	·00077	5·25	2	·00809
D	·00090	3·00	2	·00540
½ of E	·00045	1·50	1	·00068
F	·00045	5·25	1	·00236
G	·00045	7·75	1	·00349
H	·00045	10·50	1	·00472
				·04764
I as a tie,	·00045	12·00	1	·00540
I as a strut,	∞	·00	1	·00200

TABLE IX.

RESULTS FOR THE WEBS COMPLETE.

	Without I.	With I as a tie.	With I as a strut.
1st Case: Load at Top.			
1st Form of Girder, . .	·03301	·03571	·03661
2d do., .	·03224	·03494	·03584
3d do., . .	·04073	·04148	·04793
4th do., .	·04397	·04870	·04577
2d Case: Load at Bottom.			
1st Form of Girder, . .	·03096	·03366	·03456
2d do., .	·03024	·03294	·03384
3d do., . .	·03633	·03723	·04263
4th do., .	·04764	·05304	·04964

(To be Continued.)

On Single and Continuous Straight Girders. By Mr. FRANCIS CAMPIN.

From the London Artizan, May, 1862.

After a few preliminary remarks upon the impulse given to the progress of bridge building by the introduction of wrought iron as a material for that purpose, the author proposed to explain a simple and practical method of proportioning the flanches of straight girders. The amount of strain upon any part of a straight girder, might be calculated to the greatest nicety by formulæ deduced from mathematical investigations, which, however, are generally too complicated to be practically available.

The curve of strain upon a girder simply supported at each extremity is a parabolic segment, which, however, may be closely approxi-

mated by a circular segment, hence the least area of any section of the flanches may be measured on the ordinates of a curve drawn as follows :—

Find the area at the centre of the girder, from which point lay off to scale at right angles to the girder, an ordinate representing such area, then describe a circle passing through the extremities of the ordinate and line of girder. It is desirable that the vertical scale of areas be as small as possible in proportion to the horizontal scale. The area of either flanch at the centre, including loss by rivets, may be found from the expression, $0{\cdot}0313\frac{w\,l^2}{d}$, where w = load in tons per foot run, l = space in feet, d = depth in feet, the result being the area in square inches.

One span of a continuous girder may be regarded as virtually divided into two or more parts, a central part acting as a girder supported at each end, and limited in length by the points of contra-flexure, which part may be treated exactly as any ordinary single girder as described above, and one or two end parts, of which each acts as a girder fixed at one end and free at the other, bearing an uniform load, w, per foot run distributed over its length, and a concentrated load at its extremity, equal to half the total load on the central part of the girder. The area at the point of fixture being found for either flanch from the expressions, $\frac{\mathrm{w}\,x}{8\,d}$, where w = total load on half beam and on central part, d = depth of girder, x = distance of point of contra-flexure from point of support = length of half beam. All that remains to be determined is the value of x, which corresponds to a minimum area of the curve of strain.

The author then explained the process of finding x, which gives for a beam fixed at both ends, $0{\cdot}25\,l$. And for a beam fixed at one end and supported at the other, $0{\cdot}215\,l$.

In the case of a continuous girder, the values of the x's are assumed first as equivalent to one of the above quantities, and then reduced to give an equivalent of area over the points of support, whichever span such area is calculated from.

The author then proceeded to find the actual saving from the use of continuous girders, and from a calculation of numerous existent cases found that it sometimes amounted to 25 per cent. of the weight, averaging about 18 per cent.

These results were obtained from an empirical formula, for the weight of metal in a bridge, supposing single spans to be used, it is, $\frac{b\,.\,l^{2{\cdot}25}}{10{,}000}$, giving the weight in tons, b = the breadth, and l = the span, both in feet, the quantity 2·25 being found from the expression

$$n = \frac{\log.\,w - \log.\,w' + \log.\,b' - \log.\,b}{\log.\,l - \log\,l'},$$

in which w, b, l, w', b', l', are the weights, breadths, and spans for two cases; $n = 2{\cdot}25$ was the mean result of solutions of the above equations.

Proceedings Instl. Civil Engineers, April 10, 1862.

MECHANICS, PHYSICS, AND CHEMISTRY.

On the Igniting Point of Coal Gas. By Dr. E. FRANKLAND, F.R.S.

From the Journal of Gas Lighting, Water Supply, and Sanitary Improvement, No. 250.

The temperature at which coal gas will ignite under ordinary or extraordinary conditions, is a circumstance of considerable importance, both to manufacturers and consumers of gas; nevertheless, the recent explosion in Holborn, and the difference of opinion which evidently prevails, even amongst those who are intimately acquainted with the properties of coal gas, prove that the subject has not hitherto received that attention which it obviously merits. Under these circumstances, the following experiments, and the conclusions drawn from them, although they are far from exhausting the subject, may not perhaps be altogether unacceptable as a contribution to this part of the history of coal gas.

The heterogeneous mixture of gases and vapors known as coal gas, may, for our present purpose, be assumed to consist of:—

Olefiant gas and other luminiferous hydrocarbons.
Light carburetted hydrogen, or fire-damp.
Hydrogen.
Carbonic oxide.
Bisulphide of carbon.

Now, as these constituents can, to some extent, become separated from each other, under certain circumstances, it is desirable, at the outset of the inquiry, to examine separately their respective igniting points.

1. Olefiant gas, which may be taken also as the type of the remaining luminiferous hydrocarbons, could not be inflamed by a hot iron, unless the latter were heated until it appeared of a cherry-red color in the daylight of a tolerably well-lighted room.

2. The igniting point of light carburetted hydrogen was carefully determined by Davy, to whose observations I have not a word to add. He says that "light carburetted hydrogen can be inflamed by *white hot sparkling iron*, but not by iron at a red heat; it is, therefore, much less inflammable than hydrogen or carbonic oxide, and less so than olefiant gas." Bischof says that it cannot be inflamed by burning tinder, even though the tinder be strongly blown upon.

3. Hydrogen inflamed at a lower temperature than olefiant gas, but it could not be ignited by a rod of iron, unless the latter were heated to a temperature considerably beyond visible redness in a tolerably well-lighted room.

4. Carbonic oxide inflamed at a temperature somewhat greater than

that at which hydrogen ignited, but lower than that necessary for the inflammation of olefiant gas.

5. Bisulphide of carbon vapor ignited at 300° Fahr.

In some experiments recently made in connexion with the gas explosion in Wood Street, I had occasion to observe that when coal gas is allowed to mix with air contained in a space, partly enclosed, but still communicating freely with the atmosphere, such as the open gas main in Holborn, for instance, there occurred an approximate separation of the lighter from the heavier constituents of the gas; thus samples of the explosive mixture taken from such a space were found on analysis to contain olefiant gas, luminiferous hydrocarbons, carbonic oxide, and bisulphide of carbon, with but a small per centage of light carburetted hydrogen, and mere traces of hydrogen, although the two latter gases constitute, as is well known, the chief bulk of coal gas. These gases must, therefore, have rapidly made their way out of the partially-enclosed space into the atmosphere. This behaviour of the different constituents of coal gas, when the latter is slowly admitted into one end of an open main containing atmospheric air, may not inaptly be compared to that of a number of birds of different powers of flight, entering at one end of the pipe, and making the best of their way towards the opposite extremity. At every moment from the entrance of the birds, the per centage of those of swift flight would diminish near the entrance end of the pipe, whilst that of the birds of slower velocity would obviously increase in the same ratio.

The Master of the Mint has proved that the rapidity with which gases diffuse into each other, or into a vacuum, is inversely proportional to the square roots of their specific gravities; and, although there are some circumstances in the case of the open gas main, or partially-enclosed space, which would somewhat interfere with this ratio, yet, for all practical purposes, Mr. Graham's law may be assumed to express correctly the different velocities with which the constituents of coal gas would hasten to escape from the space in question.

These velocities of diffusion are as follow:—

Bisulphide of carbon,	1·00
Olefiant gas,	1·66
Carbonic oxide,	1·66
Light carburetted hydrogen,	2·19
Hydrogen,	6·23

All other luminiferous hydrocarbons existing in coal gas must have a diffusion velocity lower than that of olefiant gas.

Thus the effect of diffusion of coal gas in an open horizontal pipe, or other similar partially-enclosed space, would be to form an explosive mixture, containing chiefly hydrogen as the combustible gas, at or near the open extremity of the pipe; whilst the explosive mixture, formed near the end of the pipe where the gas entered, would contain chiefly carbonic oxide, olefiant gas, and bisulphide of carbon.

It is obvious, from experiment No. 3, that the explosive mixture, formed near the mouth of the tube, would ignite at a somewhat lower temperature than that required for the ignition of an explosive mixture containing all the constituents of coal gas. Still, even this more inflammable mixture could not be ignited below a red heat distinctly visible in the daylight of a room. It might, however, be expected that the explosive mixture, formed at the opposite extremity of the tube, would be capable of ignition at a still lower temperature, owing to the continued increase in the proportion of the highly inflammable but sluggish bisulphide of carbon, which could, doubtless, under favorable circumstances, increase to at least ten times the amount originally contained in the coal gas. In order to ascertain the effect of the presence of a considerable per centage of bisulphide of carbon vapor upon the inflammability of the constituents of coal gas, and especially of carbonic oxide and olefiant gas, the following experiments were made:—

6. Carbonic oxide was mixed with about 3 per cent. of the vapor of bisulphide of carbon, and was then allowed to issue from a jet into the air. The jet of gas readily ignited on the approach of a glass tube containing oil heated to 410° Fahr., the igniting point of the gas being probably not higher than 350° Fahr.

7. Hydrogen, containing the same amount of bisulphide of carbon vapor, ignited by contact with a tube containing oil at 420° Fahr.

Here, then, was a phenomenon which would seem to indicate the alarming possibility of the ignition, at a comparatively very moderate heat, of explosive mixtures of coal gas and air; fortunately, the next experiments entirely allay any apprehensions on this score.

8. Olefiant gas, impregnated with 3 per cent. of the vapor of bisulphide of carbon, did not inflame at a perceptibly lower temperature than when free from the admixture of the sulphur compound.

9. To the highly-inflammable mixture of carbonic oxide and vapor of bisulphide of carbon, used in experiment No. 6, a minute trace (not 0·1 per cent.) of olefiant gas was added; instantly, the igniting point of the mixture was raised to that of pure carbonic oxide.

10. A similar experiment with the hydrogen mixture (No. 7), gave a corresponding result.

Thus, the extraordinary inflammability which is imparted to carbonic oxide and hydrogen by the vapor of bisulphide of carbon, is entirely removed by mere traces of olefiant gas; and it is probable that the other luminiferous hydro-carbons contained in coal gas would produce the same effect. In order to complete this part of the inquiry, it now only remained to extend these experiments to coal gas itself.

11. Coal gas could not, even under the most favorable circumstances, be ignited at a temperature perceptibly below that described in experiment No. 4, as necessary for the inflammation of carbonic oxide.

12. When coal gas was mixed with 3 per cent. of bisulphide of carbon vapor, its igniting point was not lowered in the slightest degree.

Having thus proved that any amount of diffusion can have but a very slight effect upon the inflammability of explosive mixtures of coal gas and air, the following experiments were made to decide the disputed point, whether coal gas can be inflamed by sparks:—

13. Hydrogen was readily inflamed by sparks struck from flint and steel.

14. Carbonic oxide was also readily ignited in a similar way.

15. The mixture of coal gas and air, issuing from a wire-gauze burner, was repeatedly, and easily, inflamed by the sparks struck from flint and steel.

These results are quite in conformity with the experience of gas-engineers and managers, several letters from whom are now before me, describing the ignition of gas from the sparks elicited by the contact of a workman's pickaxe with stones, the chipping of a main, &c. The notion that coal gas will not inflame under these circumstances has, doubtless, arisen from the impossibility of so igniting the gas of coal mines; but the combustible gas existing in British coal mines has been proved, by very numerous analyses, to be light carburetted hydrogen only—no trace of hydrogen, carbonic oxide, or olefiant gas being ever present in it. Now, the igniting point of light carburetted hydrogen, as has already been explained, is very much higher than that of the other combustible gases present in coal gas, and hence, whilst the mine may be safely lighted by the "steel mill," the use of such an instrument in an explosive mixture of coal gas, would infallibly cause ignition. A word of warning as to the use of the safety-lamp in gas-works may perhaps not be here out of place. Even in the difficultly inflammable fire-damp mixture of coal mines, the Davy lamp was known by its inventor to be unsafe in certain conditions,—as when placed in a strong draft, or rapidly swung to and fro. Any degree of insecurity thus attaching to the safety-lamp in mines, is increased tenfold when it is used in explosive mixtures of coal gas, and hence it is highly desirable that the gauze of such lamps should be finer than that used in the miner's lamp, and also that the workmen should be stringently prohibited from placing the lamps in a draft of explosive gas, or swinging them to and fro, since the neglect of these precautions may easily cause disastrous explosions.

In conclusion, the results arrived at may be thus shortly summed up:—

1. Coal gas cannot, even under the most favorable circumstances, be inflamed at a temperature below that necessary to render iron very perceptibly red hot by daylight in a well-lighted room. But this temperature is considerably below a red heat visible in the open air on a dull day.

2. This high igniting point of coal gas under all circumstances, is due in a great measure to the presence of olefiant gas and luminiferous hydrocarbons.

3. The igniting point of explosive mixtures of the gas of coal mines is far higher than that of similar mixtures of coal gas; consequently, degrees of heat, which are perfectly safe in coal mines, may ignite

coal gas; hence, also, the safety-lamp is much less safe in coal gas than in fire-damp.

4. Explosive mixtures of coal gas and air may be inflamed by sparks struck from metal or stone. Thus, an explosion may arise from the blow of the tool of a workman against iron or stone, from the tramp of a horse upon pavement, &c.

5. Explosive mixtures of coal gas may also be ignited by a body of a comparatively low temperature, through the medium of a second body, whose igniting point is lower than that of coal gas. Thus sulphur, or substances containing sulphur, may be inflamed far below visible redness; and the contact of iron below a red heat with very inflammable substances, such as cotton waste, may give rise to flame, which will then, of course, ignite the gaseous mixture.

The Waste Products of Gas Works.

From the Journal of Gas Lighting, &c., No. 250.

Dr. Lyon Playfair is delivering a short course of lectures at the Royal Institution, on the progress of some of the chemical arts since 1851, and in his first two lectures, on the 8th and 15th inst., his subject was "The Distillation of Coal." He selected that subject, he observed, not because there had been any marked improvement in the manufacture of gas, but on account of the new applications of the products of gas works, which had given great value to what was formerly considered to be valueless. In the first lecture, he described the various processes of gas making, and explained the principle on which the illuminating power of gas depends. In endeavoring to illustrate the effect of enriching pure hydrogen gas by passing it through naphtha, the glass vessel in which the fluid was contained exploded when he applied a light to it, and fragments of glass were scattered in all directions, and dense brown fumes filled the lecture-room, and drove many of the ladies away. He exhibited a large and beautiful mass of paraffin, and a bundle of paraffin candles that had been made from coal-tar, and contrasted the present state of the manufacture and the use of that article with its condition in 1851, for in the Exhibition of that year, a single paraffin candle only was exhibited as a great curiosity, which had been produced from peat; whilst in the Exhibition now open, there are abundant specimens of the paraffine wax and candles made from coal-tar, the production of that article having become an important branch of manufacture.

In the second lecture, the ammoniacal products, and the dyes produced from coal-tar, were noticed. He stated that 4000 tons of the muriate, 5000 tons of the sulphate, and 2000 tons of the carbonate of ammonia are annually produced from gas liquor; and he no doubt surprised and somewhat disgusted the lady portion of his audience by informing them that their smelling bottles are filled from the refuse of gas works, and the sweepings of streets. He exhibited the process of extracting oil and naphtha from coal, and proceeded to de-

scribe and to illustrate the chemical changes that take place in the formation of aniline, roseine, and the other hydro-carbon compounds that now form dyes of all colors. Specimens of all the products were exhibited, and of silks and woollens that had been dyed by them. To illustrate more forcibly the quantities of such products derived from coal, a mass of coal weighing 100 lbs. was placed near the lecture-table; and near to it were placed the various products obtained from a mass of that size, and the quantity of wool which had been dyed by the products of the distillation of a similar mass. The chemical processes by which coal becomes converted into beautiful colors, may, he said, also be applied to extract from the mineral the smell and flavor of almonds, with which confectionery is now flavored, as a substitute for the far more dangerous dilutions of prussic acid. Dr. Lyon Playfair showed how easily silks may be dyed yellow, mauve, or magenta, by the application of the extracts from coal-tar; and, in expatiating on the great value of these new chemical processes, he observed that, by their means, this country had been rendered independent of foreign countries in the production of coloring substances, and that, in all probability, we should become the exporters of dyes, even to those parts of the world from which they have hitherto been imported.

Description of a Rivet-making Machine. By Mr. CHARLES DE BERGUE, of Manchester.

From Newton's London Journal, June, 1862.

The main feature of this machine consists in its making rivets by a continuous motion, and in its compactness and simplicity of action.

The dies for holding the blanks to form the rivets are fitted radially in a disk, which revolves on a horizontal shaft. These dies are eight in number. The heads of the rivets are formed by a cast iron header, carried by the crank fixed on a second horizontal shaft, revolving eight times for once of the disk, and so geared with it, by means of toothed wheels, as to coincide exactly with the eight dies as they successively pass before the header, at the moment of its full stroke, towards the disk. At this time the disk, carrying the dies, and the header are for a moment traveling together. The end of the crank arm, carrying the header, slides in a slot in a ring, which enters a recess in the disk, and revolves freely upon the centre pin of the disk. The inner half of this ring is turned eccentrically, and upon it a loose ring or eccentric is placed, to take the thrust of the pins which hold up the rivets during the heading, and force them out of the dies when completed. The eccentric is held in a fixed position, or nearly so, by the end of the header bar or crank arm sliding through the slotted ring, the eccentricity being set not quite opposite to the point where the heading takes place, so that the moment the header has left the die, the eccentric begins to act in forcing the rivet out. The loose ring always moves with the pin which holds up the rivet while the heading is being performed, and also while forcing out the rivet, and

thus throws the wear upon the whole surface of the eccentric, instead of confining it to the portion directly under the header.

To prevent the possibility of accident to the machine from blanks being put into the dies too cold or too large in size, the header is supported behind by a small crushing piece of cast iron, which lies free in a recess in the header bar, and is made of such sectional area as to resist the usual crushing strain required for heading a rivet, but to yield if by any accident an unyielding substance should get between the header and the die.

During the time of the header being in action, the motion of the header and the die, as governed by the toothed wheels, would not be perfectly coincident, except at the beginning and the end of the heading process. At the point where the process commences, which is a point adjustable at option, the centre line of the header as carried forward by the toothed wheels, coincides with the centre line of the rivet to be headed; then proceeding in the direction of the rotation, the rivet overruns the header slightly, and again exactly coincides with it when on the centre line or line of greatest pressure; after which the reverse action takes place as the header recedes from the die. The motions of the header and the die are however made perfectly coincident throughout by means of a steel pin inserted in the header bar alongside of the header; and eight corresponding holes to receive this pin, are bored in the circumference of the disk, side by side with the holes which contain the dies. The pin enters the hole in the disk at the point where the heading process commences; and the teeth of the driving pinion are, at the same time, partially cut away, so as to clear the teeth of the larger wheel, while the pin is in action; and then, as the pin leaves the hole in the disk, the teeth of the pinion again take up the driving action, and continue the movement of the disk. Thus the die is carried forward, during the heading process, by the pin, independently of the driving wheels, which are not required at that part of the rotation for working the machine.

The bars for making the rivets are heated in a furnace alongside the machine, and are then cut off to the required lengths by a lever-cutter, driven by a double cam on the heading shaft; thus allowing two lots of rivets to be cut for one rivet made, and so giving time for changing the bars, while still a sufficient supply of blanks is always kept cut; four to six blanks are cut off in each batch, about ten bars being kept in the furnace at once. The blanks are fed into the dies by two boys, a third boy doing the cutting. The lengths to be cut off are regulated by an adjustable gauge bar.

The machine is placed close by the side of the furnace, so that the heated bars have only to be carried about two feet distance from the furnace mouth to the cutter, and the ends cut off fall into a trough, down which they run to a convenient position for the boys who feed the dies. The finished rivets fall out below the disk into a truck placed to catch them, and are thence wheeled away. The machine is speeded to the size of the rivets to be made: thus, for one-inch rivets the disk revolves four times per minute, making thirty-two rivets per

minute; and for half-inch rivets the disk revolves five times per minute, making forty rivets per minute.

The objects aimed at in applying machinery to rivet-making are—more uniform and perfect manufacture of the rivets, and a more rapid production than by hand-making; together with independence of the risks of delay in the supply by hand work, when large quantities are required. But from the simple nature of the work, and the small margin for economy in manufacture, by the application of machinery, only a very simple and durable machine is suitable for the purpose.

The advantages found in the machine now described are—that by the continuous motion a saving of time is effected, and a larger quantity of rivets are produced in a given time; while the shocks and concussions attendant upon stopping and starting the motion, with the consequent jar, and destructive wear and tear, are avoided,—increasing the durability of the working parts. The use of the crushing piece, also, behind the header, serves as an effectual safeguard against breakage, and prevents the strain that can be put upon the machine ever exceeding the intended limit, which, for making one inch rivets, is taken at about twenty tons. The whole machine lies in a compact and convenient form, taking up a space of about 5 feet by $9\frac{1}{2}$ feet, and only about 8 feet by $9\frac{1}{2}$ feet total space, including the heating furnace.

The heating furnace is 3 feet long by $2\frac{1}{2}$ feet wide in the body, with the fire at the back end. The flame passes over the bars to be heated, and down a flue at the front end, just within the drawing-out door, thus avoiding any cooling effect upon the bars when the door is opened, and keeping up a very uniform heat.

Mr. Joy showed specimens of the rivets of different sizes made by the machine, and of the heading dies, both new and when worn out; also of the safety crushing pieces, whole and broken.

The Chairman observed that there were many difficulties to be overcome in applying machinery satisfactorily to the manufacture of rivets, and though several machines had been constructed for the purpose, few had proved durable in the working parts or perfect in the mode of making the rivets. The present machine, though not new in some of its parts, appeared in others to present novelties deserving of consideration. He inquired how long the machine had been in operation, and what had been the wear and tear of the working parts, as that was the main point in all such machines, which would often work well for a time, but afterwards were always getting out of repair, and requiring renewal.

Mr. Joy replied, that they had had two machines at work for about two years, and one of larger capacity for about one year. No wear was yet perceptible on the working parts, excepting the dies and headers, which of course had to be renewed for both hand and machine work in proportion to the amount of work done. The two horizontal shafts in the machine had been taken out and examined, and

were found entirely free from wear. The bearing and shafts were entirely cast iron, got up very true, and with such a large extent of surface that the pressure was never enough to begin wearing the metal. When the first machine was made, a heart-wheel or cam was employed for pushing the rivets out of the dies; but the ends of the jingle pins grinding against it, under the heavy pressure of forcing out the rivets, caused such an excessive wear at that part, that after a short time the cam had to be taken out for repair, and they had turned it down circular as an eccentric, and put on a loose ring, as a ready means of repairing it. This had proved so entirely successful in removing the wear, that it had been permanently adopted in the machines: the head of the pin seized the surface of the ring under the severe pressure of forcing out the rivet, and carried the ring round with it, so that there was no wear between the head of the jingle pin and the ring, while the large surface of the eccentric allowed the ring to slip round it freely without sensible wear. The cast iron crushing piece behind the header gave way occasionally with a sharp report in the ordinary course of working, particularly on first starting, before the machine got warmed into its usual working condition, the hot blanks probably being too much chilled in the cold dies; but a supply of crushing pieces was kept on hand, and a fresh one put in whenever required.

The cast iron dies usually lasted two or three days, and sometimes as much as six days. The cast iron was toughened by a mixture of wrought iron scrap, and the dies were merely cast in sand and not chilled; the die hole was drilled afterwards out of the solid, and a groove was slotted in the side of the die to receive the tightening key for holding it in the disk, but no fitting was required for the fixing the dies in their places. They had tried casting the hole in the die by means of a hollow steel spindle with water running through it, so as to chill the interior of the hole, but this did not succeed at all; and a sand core had also been tried, which was more nearly successful. The simplest and best way, however, was to cast the die solid, and drill the hole afterwards. After being worn out for one size, the dies were bored out again several times for larger sizes of rivets, before being completely worn out. The header was also made of cast iron, not chilled: cast iron was found to stand better than steel for the header, for a steel header had been tried, but it cracked all to pieces after heading a few rivets, and steel was of no use for such purposes.

Mr. E. A. Cowper had also found cast iron stand best for a similar purpose in a large hydraulic punching press, for punching out red-hot the links for suspension bridges: a link 7½ feet long and 1 foot 8 ins. across the eye, was punched out of 1 inch thickness of metal, by a cast iron punch and die, when the metal was red-hot. He had tried steel punches also, but they did not stand for punching more than half a dozen links, and then were spoilt, as the steel would not stand the frequent heating by contact with the hot iron, without cracking. Ultimately cast iron punches and dies alone were used, and lasted each

about a month in punching out the links, punching in that time probably 200 links.

In answer to an inquiry, Mr. Joy said, that two streams of water played over the disk as it revolved, to keep it cool; it must not be too much cooled, however, otherwise the machine did not work well, and the crushing piece got frequently broken; the machine was allowed to get about as hot as the hand could bear, and then it worked well.

The number of rivets made per day by the machine depended much on the form of the head: an ordinary snap or semicircular head was the best to make, but full large heads with flat tops were most difficult, requiring so much material to be crushed up to form the head; and some ¾-in. rivets that they had made with large heads, took as much as 2½ inches length of body to make the head. Any form or size of head, however, could be made in the machine by simply changing the header for one of the required shape. The number of rivets made per day by the present machine, in regular work, was about as follows:—

¾-inch	rivets,	3	inches long,	when finished,	30	cwts. or	4000 rivets der day.
¾	"	3	"	"	25	"	6500 "
⅝	"	2½	"	"	20	"	9000 "
½	"	2½	"	"	15	"	12000 "

The cost of the machine, including an apparatus for moulding and casting the dies, was about £300.

Inst. Mech. Eng., Nov. 7, 1861.

Translated for the Journal of the Franklin Institute.

On a Peculiar Mode of Producing Soap-bubbles. By M. Felix Plateau.

As a simple student of the Faculty of Sciences, I should have regarded it as premature to submit to the Academy the results of an observation which chance alone furnished to me; but I yielded to the urgent solicitations of my father, who thinks the observation curious in itself, and important in reference to a question in Meteorology.

I had gone, at the request of my father, to throw away in the garden a liquid which had served to produce the sheets and which was contained in a capsule. I wished to try, by throwing it obliquely, to spread it out in a sheet: I did, in fact, obtain a sheet, but to my surprise I saw it convert itself into a hollow bubble from 3 to 3·5 inches in diameter, falling slowly. I repeated the experiment a great number of times, using simply soap-suds, and I found it to succeed certainly: only, there almost always formed several bubbles, sometimes as many as fifteen: their diameter, which in the largest reached from 3 to 3·5 inches, was less in proportion as they were more numerous.

The conditions which appeared to me to be the best, were as follows: Take a vessel of basin shape and about 6 inches in diameter: (you may succeed, but less easily, with vessels of other forms and dimensions:) the liquid must be in considerable quantity; throw it at an angle of about 45° with the horizon, turning rapidly around, so as to produce as extensive a sheet as possible. The liquid which has

given me the best results is 1 part of Marseilles soap in 40 parts of water.

In order to unravel more clearly the manner in which the phenomenon took place, I observed from an upper window, while another person placed below, performed the experiment as I have explained.

I thus discovered that the liquid sheet of very irregular form and indented on the edges, was resolved along the edge into numerous full drops, while the rest was generally torn into several parts, each one of which closed up rapidly so as to form a complete hollow bubble.

My father sees in this phenomenon, an argument in support of the vesicular state of the vapors in clouds. In fact, one of the principal objections urged against this hypothesis consists in the impossibility of conceiving how the molecules of the vapor could, whilst it was passing to the liquid state, aggregate together so as to form envelopes enclosing air. Now, we see that such an aggregation in closed envelopes is not immediately necessary; it is sufficient that the molecules of water should unite in open sheets of any figures and curvatures: each of these lamellæ will immediately close so as to give rise to a vesicle. Doubtless the character of these lamellæ is itself not easily conceived; but it appears at least much more admissible than the entire formation of the vesicles.—*Cosmos.*

Quicksilver Mines of New Almaden, California.

From the Lond. Mining Journal, No. 1357.

The latest advices from California state that the quicksilver mines of New Almaden, which were so long a subject of litigation, are now again in full operation, and that their yield is immense. According to one account, the workmen have reached the depth of 18 ft. below the surface, and the quicksilver is still found in little globules, so thick that a stream almost follows the stroke of the pick. The *San Francisco Mining and Scientific Press* says—"There have been many recent discoveries of rich cinnabar in various portions of our prolific State, in consequence of the long stoppage of the great New Almaden Mine, owing to tedious and vexatious litigation. This mine, however, is at last in full operation again, and its annual product of over 1,000,000 lbs. of quicksilver will again be in the market. The New Almaden, New Idria, Enriquita, and Gaudaloupe Mines of Santa Clara county, with the many recently discovered and only partially worked cinnabar veins of Napa and Sonamo counties (which contain liquid quicksilver), already turn out some 4,000,000 lbs. of quicksilver per annum—an amount nearly large enough to supply the world—and, doubtless, when these latter shall have been more thoroughly opened, the yield will reach the high figure of 8,000,000 lbs., the value of which, at the rate of 30 cts. per lb., would be no less than $2,400,000; and this we think is a moderate estimate. Hittell sets the average aggregate annual yield of the four great Santa Clara Mines at 3,510,000 lbs., but it has reached as high as 4,275,000 lbs.; and as

they are by no means worked to the best advantage, we may safely look for largely increased returns as the operations of the companies are extended. Lack of experience, money, and harassing litigation, heretofore have very much retarded these operations; and we shall not be at all surprised if within the next ten years the annual yield from Santa Clara doubles itself. If the accounts of discoveries in Napa and Sonoma counties approach the truth, then we have a still richer district there, whose yield can hardly be computed. Accounts recently received from Washoe also speak of rich cinnabar veins discovered; we hope the reports are true. With so many mines, and such vast yields, we may safely predict that the wholesale price of quicksilver will constantly decrease until it reaches 8 or 10 cents per lb. Quicksilver will then be used with a more liberal hand in the search for gold and silver, and many other advantages to the world will arise from its cheapness."

On an Application of Giffard's Injector as an Elevator for the Drainage of Colliery Workings. By Mr. CHARLES W. WARDLE, of Leeds.

From Newton's London Journal, June, 1861.

The apparatus described in the present paper was applied by the writer to meet the special requirements of the working of a portion of the Kippax Colliery, near Leeds, where it has been in constant operation for the last eight months. It is a self-acting apparatus for raising the water for drainage of a portion of the pit workings, and has completely answered its intended purpose; and a similar application may be of service in other special cases, where the cost of fuel is not a consideration, but a simple and inexpensive apparatus is required, not needing attendance in working. In the present case, a small portion of the colliery (about 2 acres) was required to be worked out, which was lying below the drainage level of the pit, and at a considerable distance from the shaft; and as the extent to be worked was so limited, it did not allow of the erection of a special pumping engine, and hand pumping was employed to raise the water a height of 27 feet to the upper level, which was drained by an engine. This mode of draining was continued for two years; two shifts, of two men each, being employed constantly at the last, with a 3½-inch pump; but they were not able to keep down the water in the lower workings, as it had increased in quantity, and some other less expensive and more efficient means was required to enable the rest of the coal to be worked out.

An application of a Giffard's injector as an elevator was proposed and carried out by the writer. The steam pipe used was a wrought iron pipe, of 1½ inches bore, with screwed joints. It was carried 60 feet from the steam boiler to the shaft, descended the shaft 243 feet, and then passed along an inclined heading 730 feet long, and falling 27 feet to the elevator, which took the water at that level and discharged it by an iron pipe of 2 inches bore, carried a distance of 300

feet up another inclined heading to the main drainage level at a height of 27 feet above.

The elevator is fixed in a cistern sunk in the water, so that the inlet pipe is at the water level. The elevator is a modification of a small-sized injector, made in the simplest form for the sake of cheapness. It consists of a fixed steam jet, with a brass nozzle of $\frac{5}{16}$-inch aperture, fixed in a cast iron casing, without any means of altering the position, with the steam pipe connected at the top, and the discharge pipe at the bottom, the whole being closed without any overflow; the discharge aperture is tapered gradually in both directions to $\frac{3}{8}$-inch bore at the throat.

In consequence of the great length of the supply steam pipe (1030 feet from the boiler to the elevator), provision had to be made for constantly carrying off the condensed water deposited in the pipe, in order to insure the elevator being constantly supplied with tolerably dry steam, as the entrance of water with the steam would stop its action. This is effected by passing the steam through the top of a depositing box, 10 inches diameter and 3 feet deep, from the side of which the water flows off into a self-acting water trap. This trap consists of a closed cylinder, containing a copper cylindrical float, 8 ins. diameter and 8 ins. deep, open at the top, and guided by a centre tube sliding up the small pendent pipe. This pipe is prolonged outside over the side of the vessel, and serves as the discharge for the accumulated water; its lower end is closed by a small conical valve fixed on the bottom of the copper float, which keeps it closed until the water has accumulated in the trap outside the copper float so much as to flow into the float and sink it. The conical valve is thus opened, and the water contained in the float is expelled through the discharge pipe, by the pressure of steam upon its surface; the float again rises and closes the pipe ready for another charge.

The temperature of the heading, through which the steam pipe is carried, is about 72° Fahr., and the steam pipe is clothed for about one-third of its length, throughout the portion from the boiler to the bottom of the shaft, with a coating of tarred felt wrapped round it, and the remaining portion in the pit is wrapped with haybands. But with the great length of the pipe (1030 feet), and its small diameter of $1\frac{1}{2}$ inches, this clothing is not sufficient to prevent a very considerable amount of condensation taking place; and there is a constant discharge from the water trap of about 3 gallons per hour during the working of the elevator, a discharge taking place at successive intervals of about a quarter of an hour. This serves quite efficiently for keeping the steam supplied to the elevator free from water, and the elevator continues working uninterruptedly for many hours together. When the supply of drainage water is sufficient, it works continuously day and night without stoppage; it does not require any attention in working; and is started simply by turning on the steam at the boiler at top, when the elevator starts working at once. There is no valve in the discharge pipe, so that the pipe becomes emptied each time that the elevator is stopped working, the water running back through the in-

strument, and out at the inlet pipe: there is consequently no pressure of water to be overcome at starting; and the elevator always starts working at once, when the supply water is up to the level of the inlet, but not if the water has to be lifted in the inlet pipe. All that is required in starting the elevator afresh is to blow through, by turning on the steam for two or three minutes to warm the pipes; and then, after shutting off the steam for a few seconds to allow the condensed water to drain off, the apparatus is started at once in full work by turning on the steam again.

When the apparatus was first set to work, the depositing box had not been applied, and the action of the elevator was soon stopped by an accumulation of rusted scales from the interior surface of the wrought iron steam pipe. But the addition of the depositing box completely removed this difficulty, and the box has never required opening during the eight months it has been in work.

The pressure of steam at which the elevator is regularly worked is 34 lbs. per square inch above the atmosphere at the instrument, and it will keep working down to about 28 lbs. pressure, when it stops. A difference of pressure of 13 lbs. per inch constantly exists between the two ends of the steam pipe—the working pressure in the boiler being 47 lbs. per square inch, in consequence of the large condensation in the pipe, and the resistance caused by its small diameter. The elevator accordingly stops working when the boiler pressure is lowered to about 40 lbs. per inch.

The temperature of the delivery water at the further end of the discharge pipe is 94°, that of the supply water being 58°; and the quantity discharged by the elevator is 780 gallons per hour, raised a height of 27 feet. The consumption of rough coal slack, for generating the steam supplied, is about 1½ cwts. per hour; the boiler is a plain cylindrical one, 4 feet diameter and 30 feet long, with oven setting; and supplies steam also to the winding engine for the pit; but its consumption of fuel was taken separately in the night, when supplying steam to the elevator alone, the fuel being only refuse of slack from the pit, of a very inferior and dirty description, for which there is no other use. Consequently, the only expense incurred in the drainage of the workings by the elevator, besides the little extra wear of the boiler, is the cost of attendance for firing the boiler about once per hour during the night, when the winding engine is not required to be worked. There have been no expenses for repairs of the apparatus, the elevator never having been opened since first put in its place, except on the occasion previous to adding the depositing box soon after starting, as before mentioned.

The following are some of the cases where the elevator seems to be applicable with advantage. Where fuel is very cheap, as at a pit's mouth, and where the small coal is burnt simply to get rid of it. Where steam is blowing to waste, as in forges at night, when the production of steam continues without occasion for its use to the same extent as in the day, and often its blowing off at night is a nuisance: this application of the elevator has been made at Messrs. Sharp,

Stewart & Co.'s works, at Manchester, to fill up the tanks during the night for the day's supply, by making use of the waste steam previously thrown away. Where warm water is of value in the top cistern, as in the case of railway tanks and some factory purposes. Where the supply is seldom needed, and it is desired to save the first cost and maintenance of a pumping engine, and also the attendance of an engineman. Where absolute certainty of having the water lifted is required, over and above all considerations of expense, as in keeping the tuyeres of a blast furnace cool: Messrs. Schneider, Hannay & Co., of the Ulverstone Hæmatite Iron Works, are arranging to attach elevators to supply the cisterns that furnish the water to the tuyeres of their blast furnaces, being unwilling to depend on their pumps, which sometimes fail, whereas the elevator never fails; in this instance, also, there is an abundance of steam raised by the heat of the waste gas. Where frosts prevail, and pumps suffer from ice, the elevator, like the injector, gets itself into working order upon the admission of steam, even when the whole instrument is a mass of ice, the heat of the steam gradually thawing the entire mass without the possibility of doing harm. Where the boiler is far removed from the elevator, as in the case of the drainage of colliery workings described in the present paper: with the elevator no attendant need be sent to start it or mind it.

—

The Chairman observed, that the elevator was a very interesting application of one of the most ingenious inventions, and was likely to be particularly useful for special cases in mining districts, where fuel for raising steam was of so little cost, and the great considerations were simplicity of construction and certainty of action, without requiring attendance.

Mr. C. W. Siemens said, that although the injector was very beautiful and economical in action where water had to be raised and also to be heated, as in feeding a boiler, it was remarkably deficient in respect of economy when employed simply as an elevator for raising water, where the water was not required to be heated. This was shown in the experiment which had been mentioned, where the water was raised only 36 feet high in being heated 26°; but the perfect equivalent of heat, as established definitely by Joule's investigations, and others subsequent, was that the heat required to raise 1 lb. of water 1° in temperature would raise 1 lb. a height of 772 feet, and 1 lb. heated 26° would raise 1 lb. a height of 772×26 or 20,072 feet; hence the economy of the elevator was as 36 to 20,072, or only $\frac{1}{558}$th of the theoretical perfect duty of the heat. A very good pumping engine realized $\frac{1}{8}$th of the theoretical effect, and ordinary steam engines realized $\frac{1}{10}$th to $\frac{1}{14}$th, and were consequently 40 to 66 times superior in duty to the elevator. The elevator was therefore economically applicable only where fuel was no object, or as an injector where the heat came in again usefully, as in feeding a steam boiler.

The injector indeed, although inferior to a pump in mere propelling power, he considered the most perfect instrument for feeding boilers,

so long as the supply water was cool enough to allow it to work; for then all the heat imparted to the water was returned into the boiler without any waste, whereas in using steam to work a pump, the larger part of the heat was wasted by being thrown away with the exhaust steam. The injector, however, would not be economical if the supply water could be heated by other means free of expense, since its action required the supply water to be kept cool.

Inst. Mech. Eng., Nov. 7, 1861.

A Paddle-Wheel without Floats.

From the London Mechanics' Magazine, November, 1861.

SIR—I witnessed a few days ago some curious experiments on the action of plain disks, when caused to revolve as the paddle-wheels of a model vessel. To my great surprise, these wheels, formed of three, four, or five smooth thin plates, without any ribs, spokes, vanes, or other projections, propelled the model readily. This model was two feet long, and the disks were three inches in diameter, and set about half an inch apart on the paddle-shaft. I am informed that experiments on a larger scale with a steamer 90 feet long, have confirmed the conclusion, that for an equal number of turns a vessel would be propelled faster by the disk wheels than by those with floats, while the advantages of little commotion in the water, and great simplicity in action, are much in favor of the new plan. I call it "new," because I am not able to see any similar proposal in the Record of the Patent Office, or the notes of the abridgments of patents in propulsion, and I think it will be interesting to your readers to consider what are the precise means by which a wheel deprived of floats has any, not to say a more powerful, action on the water than a wheel with floats, as usual. Yours, ROB ROY.

TEMPLE, Nov. 12.

Spiral Fluted Nails.

From the London Builder, No. 969.

A company has been formed for bringing into practical use the spiral fluted nails, an invention of Mr. W. Wigzell, of Exeter. Mr. Wigzell recently exhibited these nails at Devonport. Mr. John Weary, builder, Mr. Ash, surveyor of the manor, and several other gentlemen, says the *Devonport Independent*, were present. The inventor proved the advantages of the nail in a variety of ways: he first drove one of about 2½ inches long into a piece of 2½-inch deal to within a space little more than the diameter of the nail of the "end-grain," to show that the nail so successfully cleared its way as in no case to cause the wood to split; he next drove a nail into a piece of hard, knotty oak; he nailed two pieces of 1½-inch board together by one nail at one end, and, with a leverage of 2 feet, a strong man present had great difficulty in separating the two pieces; with a nail at each end, a wedge and several powerful blows with a heavy hammer were necessary to

get them asunder. The holding qualities of the nail were exemplified by the several tests. The nails, however large they may be, or however hard the wood on which they may be used, require no holes to be made before driving; the spiral point causes the nail to revolve as it advances. Perhaps the tenacity with which they hold in wood would be one disadvantage against their use in all purposes, especially in the nailing of floors where removal is frequently necessary for laying on gas; but this inconvenience the inventor has obviated by the manufacture of another nail, with a slot or screw head, by which it can be turned out with a screw-driver even more readily than a screw, whilst it could be driven into the flooring with less than a quarter of the labor caused by the use of screws. All present, adds our authority, seemed to concur in the great superiority of the nails over those ordinarily in use. They can be manufactured as cheaply, if not cheaper than common nails. The nails are manufactured by machines invented by Mr. Wigzell, each of which will twist at least 4000 per hour; and the plain nails now in use can be twisted by other machines, also invented by Mr. Wigzell, at the rate of 16,000 per hour. Premises have been taken at Topsham for the purpose of manufacturing these nails.

On Cast Iron. By Mr. John M. Oubridge, of Messrs. Simpson's, Pimlico.

From Newton's London Journal, June, 1862.

The author said that, generally speaking, there were no fixed rules or formula laid down for the guidance of those who conducted the processes of founding castings of iron, and foremen of foundries were consequently left much to their own individual ingenuity and talent, in conducting the work entrusted to them. To judge rightly of their responsibility, let his hearers take into account the varieties of pig iron in use; how diversified were they in their nature? The density of pig iron frequently varied to the extent of 12 lbs. in the cubic foot, its cohesive strength, of course, ranging proportionably. In point of crystallization again, what diversity was found! Some iron, for example, made from the black band ores of Scotland, was remarkable for the large size of its crystals. In the process of crystallization, this iron did not require such an amount of supply as the Staffordshire irons. Then, if a comparison were instituted between the black band ore and the red hæmatite ore as to cohesiveness, it would be found that the first possessed little of that qualification—at all events until it had been frequently re-melted; whilst the hæmatite possessed it in a remarkable degree, when melted at once from the pig. The fusibility and the fluidity of iron differed, too, exceedingly. The rich black band iron of Scotland retained its fluidity much longer than either the Staffordshire, Welsh, or Cleveland irons. Thus, coming to the question of purity, the "Bowling" pig iron was distinguished by its firm grain, the "Blaenavon" for its freedom from dross, and the "Old Park" for the fine polish of which it was susceptible. In using other kinds of

iron it was found that, while in a fluid state, impurities were constantly rising to the surface; and as yet no laws have been laid down for the guidance of the practical founder, who was obliged to resort to the "rule of thumb," in default thereof.

Scientific men, chemical and otherwise, were much at fault in respect of these and other matters connected with cast iron. The mixtures of various irons for producing castings suitable for the diversified purposes to which they were put, were only empirically known. This was a wide field for scientific research, and the author would be glad to see more scientific laborers employed in that field.

To some extent employers, perhaps, were responsible for the fact that iron founders, as a body, had to grope as it were their way forward through difficulties and failures, both from their inattention to the subject and their engagement of men ignorant of metallurgical science to conduct their foundries. When the serious and disastrous failures of castings in iron were taken into account, surely little enforcement from him was necessary to demonstrate that the making of such castings was not understood. There was a hap-hazard about the process which too often was revealed by the sacrifice of human life. The fatal beam of the Hartley Colliery engine was a case in point. It was an open sand casting, of irregular thickness, as regarded its bosses and ribs, and the power of supplying the requirements of crystallization by heads of pressure, was absent at the time of its formation. Every one whom he was addressing probably knew that a plate of iron cast in open sand was one-third weaker than when cast covered, and with a sufficient head to give it uniformity. It was strange that the Hartley beam had been cast without this simple precaution having been taken, and he thought that, for the honor of the founding trades generally, it was desirable to mention the fact.

The principles which should guide the founder, in the method of supplying crystallization, seemed to be but little understood. Some year ago, when in Liverpool, several large rolls for sugar mills were to be produced in the foundry with which he was then connected. Repeated failures in making sound castings occurred, and all the "heads" and all the "feeding" they could give seemed to be of no avail. The want of homogeneity soon became visible in the latter. Half a dozen rolls were condemned. On being consulted by the head foreman on the matter, he suggested that instead of four heads, one above each arm of the roll, the mould should be made some 18 inches higher, and an annular head, double the thickness of the roll, be furnished. This plan was adopted, and no more failures occurred. The same system he now always applied in casting large cylinder covers, the bosses of engine beams, and other works of a character which demanded homogeneity and perfect crystallization.

Hitherto, that expensive schoolmaster, Experience, had been the founder's only teacher. Science could scarcely be said to have shed its "rays divine" upon them, whilst all around them its beams were illuminating dark places, and "telling" upon almost every department of social existence. What they wanted was, a special treatise on

practical iron founding. Every other branch of manufacture almost, had its text book, its organ or organs of information—the practical iron-founders had none. Some few of the questions which a "Manual of Iron Founding" should resolve he would enumerate:—The most improved method of moulding. The different qualities of iron, and how to judge them. The effects of sulphur on iron. Of manganese. Of arsenic. Of sulphate of lime. Of sulphate of copper. Ores, and how to distinguish those best suited for particular purposes. The effects of crystallization. Of expansion and contraction. The different kinds of coke, and its effects upon iron. How to manage a furnace. The methods of smelting iron. And lastly—though we are only on the threshold of queries—the proportions of different kinds of iron to be used for particular purposes—and why? He trusted that this hint for the publication of a manual might be adopted, if the right man or men could be found to compile it.

Lond. Assoc. Foremen Eng., April 5, 1862.

India-rubber Varnish.

From the Lond. Mechanics' Mag., September, 1861.

That india-rubber dissolved in various liquids yields a good varnish is well known; but in general they are too viscid for delicate purposes, and are only good for making stuffs water-proof. India-rubber liquefied by heat, dissolved in oil of coal-tar, or drying linseed-oil, does not give a varnish of sufficient fluency, or free from smell. Moreover, a considerable quantity of india-rubber remains undissolved in a gelatinous state, suspended in the liquid, so that the solution is never clear. Dr. Bolley has recently published some remarks on this subject which may be useful. If india-rubber be cut into small pieces and digested in sulphuret of carbon, a jelly will be formed; this must be treated with benzine, and thus a much greater proportion of caoutchouc will be dissolved than would be done by any other method. The liquid must be strained through a woolen cloth, and the sulphuret of carbon be drawn off by evaporation in a water bath; after which the remaining liquid may be diluted at will with benzine, by which means a transparent but still yellowish liquid will be obtained. A more colorless solution may be prepared by digesting india-rubber cut into small pieces for many days in benzine, and frequently shaking the bottle which contains it. The jelly thus formed will partly dissolve, yielding a liquid which is thicker than benzine, and may be obtained very clear by filtration and rest. The residue may be separated by straining, and will furnish an excellent waterproof composition. As for the liquid itself, it incorporates easily with all fixed or volatile oils. It dries very fast, and does not shine, unless mixed with resinous varnishes. It is extremely flexible, may be spread in very thin layers, and remain unaltered under the influence of air and light. It may be employed to varnish geographical maps or prints, because it does not affect the whiteness of the paper, does not reflect light disagreeably as resinous varnishes do, and is not subject to crack or come off in

scales. It may be used to fix black chalk or pencil drawings; and unsized paper, when covered with this varnish, may be written on with ink.—*Galignani.*

For the Journal of the Franklin Institute.

Remarks on Force, Motion, and Inertia. By JOHN C. TRAUTWINE, C. E., Philadelphia.

MR. EDITOR:—

At the risk of appearing presumptuous, I venture to submit for publication in your Journal, certain views of my own on the subjects of force, motion, and inertia. Inasmuch as they are, to a considerable extent, at variance with those expressed by the standard authors, as well as by the scientific friends to whom I have at various times communicated them, I lay them before you with some diffidence; and should not do so at all, were it not for the hope that if they are erroneous, some competent reader may have the kindness to rectify them through the same medium.

Notwithstanding the stress which writers lay upon the necessity of a clear comprehension of inertia, and its effects, it appears to me that their own ideas on the subject are exceedingly vague: the fault may, however, be on my own part.

First, Newton defines it (see Barlow's Math. and Phil. Dict., "vis inertia,") to be a certain "vis," or "a *power* implanted in matter, by which it *resists* any change endeavored to be made in its state; that is, by which it becomes difficult to alter its state either of rest or motion." "It agrees with the *vis resistendi*, or power of resisting, by which every body *endeavors, as much as it can*, to persevere in its own state of rest or motion." Barlow adds, "The vis inertia (the same great author elsewhere observes) is a passive principle, by which bodies persist in their state of motion or rest," &c. Barlow himself makes no comment on the self-evident incongruity of these definitions; thereby tacitly acknowledging that he has nothing further to add on the subject: if indeed he may not be regarded as endorsing both, which I infer he does.

Now I cannot see how a passive principle can enable a body either to persist, or to resist; and it seems to me that Newton's application to the same thing, of definitions so entirely antagonistic, as passive principle, and resisting power, evince uncertainty in the mind of the great philosopher himself, as to what he was writing about.

Prof. Rankine, in his "Applied Mechanics," says that the inertia of a body is the same thing as its mass. Now most authors define mass as being the quantity of matter in a body. Prof. Rankine evidently does not employ the word in this sense; but he does not intimate *what* he means by it, further than that it is another name for inertia; and inertia, another name for it. He states however that it is a certain quantity (of what?) proportional to the weight of the body; and also that it is proportional to the force which is required in order to produce a given definite change in the motion of the body;

but what proportion or per centage it bears to the force, is not alluded to. Still, I infer, that we may assume at least, that Prof. Rankine admits the existence of something or other, called inertia, in matter: and this is sufficient for my purpose, which is to establish, if I can, that there is nothing of the kind. My own opinion is, that it is the "moving force itself which is required" to be *given* to matter, in order that matter may *have* it, and be moved by it, that is sometimes mistaken for something or other, which has been called inertia.

Weisbach, in his "Mechanics of Engineering," vol. i., p. 52, says, "Inertia is that property of matter, in consequence of which it can, of itself alone, neither acquire nor change motion. Inasmuch as a development of force takes place at every change in the motion of a material body, in so far, inertia may be ranked among forces." On p. 57, he says, "The *force* of inertia, which manifests itself when changes in the velocity of inert masses occur," &c. And on p. 60, "If we lift a body slowly enough to allow of our neglecting its inertia," &c. These quotations will perhaps suffice to prove that Weisbach also admits the existence in matter of something or other, either a force, or something like a force, which is called inertia; that it manifests itself at times; and that its influence is such that we must be careful how we neglect it.

In that excellent book, "Cooke's Chemical Physics," p. 32, we read that "inertia is that quality in matter, by which it is incapable of changing its state, either of rest or of motion;" and on p. 33, "If when a body has acquired a given velocity, the force ceases to act, the body will continue to move on with the same velocity, and in the same direction which it had when the action of the force ceased. This, which is a necessary consequence of the principle of inertia," &c. Here we have an unmistakeable admission, that inertia, if not itself a force, still can perform the same functions that force does; inasmuch as it can *maintain* motion in a body, after the force which imparted that motion has ceased to do so. Now I have always thought, and still think, that force, and force only, can either impart or maintain motion in matter; and that the instant the force which imparts the motion ceases to act, the motion also ceases. If the idea expressed in the last quotation be correct, then I conceive that inertia should be said to assist motion, rather than to resist it, as most writers maintain. But inasmuch as I eschew inertia altogether, I cannot admit that it has any effect *whatever* on motion.

In Silliman's "First Principles of Philosophy," p. 15, inertia is called a passive property of matter; and it is added, "matter has no spontaneous tendency either for rest or motion:" and we find in p. 16, that this absence of all such tendency in matter is every day proved by "familiar instances of a tendency in matter to continue in a state of motion!" On p. 16, we also are told distinctly that inertia is a *resistance* to force: and that "motion may be continued in a heavy body with a fraction of the force necessary to begin it." Also, that "time is necessary to *overcome* inertia." Also, that "inertia is the most important general property of matter." What is a tendency, but force?

and how do we prove the absence of tendencies, by indicating their presence?

Many authors first define inertia as being an entire absence from matter, of all resistance to force tending to change its condition of rest, or motion; but afterwards speak of the force necessary to overcome inertia. Can force be necessary to overcome *no* resistance? I admit the absolute necessity of force, either for re-acting against force, or for imparting velocity to matter; but not for overcoming *no* resistance, or *no* force.

Also, when a carriage starts suddenly into rapid motion, the bodies of its occupants are said to be thrown backwards by their inertia, or resistance to motion; or if the carriage stops suddenly, they are thrown forwards by their inertia, or resistance to rest. So it appears that this non-resistance of inertia to either motion or rest, actually resists both. This example will be referred to again.

That highly practical and learned French philosopher, A. Morin, in his "Mechanics," (recently and ably translated into English, by Jos. Bennett, C. E., of Brooklyn,) endeavors to loose the gordian knot, by asserting plumply (pp. 8 and 9), that inertia *is a resisting force, inherent in matter;* and he even describes an experiment for measuring its amount in pounds.

He says, "Since some outward action is always necessary to change the state of motion of a body, it is apparent that the body must oppose a certain resistance, arising from its inertia; or, as Newton defines it, the force residing in matter (the inseated force) is the power of its resistance." "A body exerts this force whenever it changes its actual state of motion, so that we may consider it under two different aspects; either as resisting, in so far as the body is opposed to the force which tends to change its state; or as impulsive, inasmuch as the body itself makes an effort to change the state of the obstacle which resists it. Thus we give to the force residing in bodies, the very expressive name of *force of inertia.*" "Newton's Principia, vol. i. p. 2." So Mr. Morin leaves us in no doubt of his conviction that inertia is actually a *force* residing in matter.

With due deference, however, to Mr. Morin, it appears to me that the various phenomena of force and motion admit of more easy and satisfactory solutions, by ignoring the very existence of this mysterious inertia altogether; indeed, up to this time, I have found it utterly impossible to conceive of such a property in matter; and unless stronger arguments be adduced in support of its existence than I have hitherto met with, I shall be compelled to adhere to my skepticism. Before concluding this communication, I hope to be able to show that inertia has nothing to do with the result of Mr. Morin's experiment.

My own opinion of inertia is, that it is simply a *word* expressive of the entire *absence from* matter of all power whatever to hear, see, taste, smell, feel, talk, or control its own movements: an entire absence from matter of all resistance whatever to extraneous force tending to change its state of motion, or rest; in other words, that matter has no soul, mind, or will.

I conceive that the expression, "matter *has*, or *possesses* inertia," is improper; and that matter merely *is* inert. Matter never moves; never ceases to move; but force, and force only, either moves it, or stops it. A *property* is something possessed; not something not possessed. We do not say that the *absence* of sight is a property of matter. Is it not fully as improper to call the *absence* of resistance to moving force, a property? (I will here state by way of parenthesis, that the expression, "in my opinion," recurred so frequently in my manuscript, that I cancelled it. The reader will please imagine it to be restored, before whatever would otherwise appear to be a dogmatical assertion on my part. I am not trying to teach, but am soliciting information.) There is no such thing, force, property, principle, quality, possession, or attribute *in matter* as inertia. The introduction of the word, in connexion with such ideas as are suggested by the use of any of these terms, is not only unnecessary, but positively prohibitory of a clear understanding of the simple relations which exist between matter, force, and motion. Nothing could more clearly prove its injurious tendency, than the fact that it has plainly deceived nearly every writer into a vague conviction that inertia is a *something residing in* matter; and that this something does, to some undefinable extent, interfere with what I conceive to be the perfectly harmonious and unalterable relation which exists between extraneous moving-force, and motion. It is evident from their writings that they conceive that when moving force is imparted to matter, the matter would move more obediently to it, or would have more motion, were it not for this inertia. They are constantly ascribing such and such phenomena to inertia, instead of to force. It is almost needless to remark, that wherever I employ the word force alone, I allude to extraneous force, and not to molecular forces.

It would be well before proceeding further, to define *motion*. In the ordinary acceptation of the word, it means simply change of place, or position; and *velocity* is the speed with which the change takes place. But in a scientific sense, the word motion as employed in relation to matter, involves in inseparable connexion the quantity of matter moved, and the velocity with which it is moved. In this scientific sense, *motion* means precisely the same as *momentum*, or as *moving-force;* that is, force which causes velocity. I shall employ the word *motion*, only in the scientific sense; and, for the ordinary acceptation of it, shall substitute *velocity*. The *quantity* of matter in a body, is equal to the *weight* of the body.

Since, then, the motion or moving force of a body is made up of the weight of the body and its velocity combined, it is evident that the quantity of motion in any body is represented by the product arising from multiplying its weight by its velocity. We usually estimate weight as well as force, by pounds; and velocity, by feet per second: therefore, if a body weighing 10 lbs. has a velocity of 6 feet per second, we say that its motion, momentum, or moving force, is equal to

$10 \times 6 = 60$ *foot-pounds* per second. Also, if a body weighing 1 lb has a velocity of 60 feet per second; and another weighing 30 lbs

has a velocity of 2 feet per second; each of them is said to have a motion, or moving force of 60 foot-pounds per second; because $1 \times 60 = 30 \times 2 = 60$; the same as in the first example. Therefore, each of these three bodies possesses the same quantity of motion, or moving force: or, in other words, the same quantity of moving force which would give to 10 lbs. of matter a velocity of 6 feet per second, would give to 1 lb. a velocity of 60 feet per second; or to 30 lbs. a velocity of 2 feet per second. Therefore, no change in the quantity of motion takes place when its velocity is diminished, if its weight is increased in the same proportion; or, when the velocity is increased, if the weight moved is diminished in the same proportion.

If a certain degree of velocity is required in a body of matter, a proportionate amount of extraneous moving force must be applied to that body; not to *overcome inertia*, or any thing else; but simply to *impart* the velocity. In other words, since matter is inert, and therefore cannot move itself, we ourselves, if we wish it *to have* motion, must *give* it that motion. It will not *resist* it in the slightest conceivable degree, but will move in the most passive obedience to it; unless prevented by some other extraneous force re-acting in the opposite direction.

Even if we give to the body less motion than we wish it to have, and less than it is ready at all times to accept, it will still exhibit its inability to resist or re-act against even that small gift; and will move with a velocity in exact proportion to it. We must however blame *ourselves* only, that it has not accepted the quantity we wish it to have; and not say the *matter* hath a devil, called inertia, that resists. Resists what? Surely not the motion we *give* it. Can we with propriety say that it resists motion which we do *not* give it? Would we say that a ten gallon keg *resisted* four gallons of water, merely because we put but six gallons into it? Does a beggar *resist* a shilling, because we *give* him only a sixpence? The cases are perfectly parallel. Now here is the rub. The matter does *not* resist moving force; the philosophers say it *does*, and moreover that it does so through the agency of a certain property of matter called Inertia.

What is force? Nobody knows. We apply the *name* force to that agency or influence, whatever it may be, or whatever its origin, which when it is applied *to* matter, *enters unresistedly into it;* diffuses itself throughout it; and not only gives it velocity, but continues in it, and maintains that velocity unchanged, in the same straight direction, until prevented from so-doing by the *re-action* of some other extraneous *force* which it may encounter on its way.

This *unresisted entrance* of force *into* matter *to* which it is only *applied*, and its imparting of *velocity* to that matter, appear to me to be among those mysteries, the comprehension of which transcends the limits of the human mind; and recall to our overstraining imaginations the words of the inspired penmen: "God's ways are higher than our ways, *and past our finding out.*" It is to the difficulty of fully believing or realizing this mysterious property of force, that I ascribe

much of the inertia trouble. Who can form any idea of the process by which galvanic force, when *applied to* the telegraph wire, *enters* that wire, and travels along it with lightning speed?

To many readers the suggestion that the entrance of force into matter is unresisted, will no doubt appear absurd; for if it is once admitted, away goes their resisting inertia of matter. They say we actually *feel* the resistance of matter whenever we strike a body; and feel it so distinctly and often so painfully, that to pretend to deny its existence is but little short of insanity. Now, when we strike a body, we certainly do invariably experience an undeniable resistance or re-action; not from the body, but from the *forces* of gravity, friction, atmosphere, &c., which are acting on the body at the same time; and which *forces* we *must re-act against*, or hold in equilibrium before we can impart motion or moving force to the body. Nay, we should even experience a sensation *resembling* that produced by a resistance were we to strike a body on which no other force was acting at the time, and which would then *unresistingly* move on from the effect of our blow. What causes *this* sensation? Why simply this. In order to impart, or to make a present of a quantity of motion to the body, we have taken that motion out of *our own* hand, by an effort of *our own* will, and have *volutarily given it away* to that inert, unresisting, passive, obedient recipient of the gift, the body of matter. Since we have given away that motion to the body, it is self-evident that that motion no longer remains in our hand. That very identical force which was carrying our hand forward, is now engaged in carrying the body of matter forward; and it was our own voluntary giving away of that force to the body, which prevented it from any longer moving the hand, and thus produced in our hand the same sensation as if the body had *resisted* it, and *compelled* it to part with its moving force. The *sensation* is the same, whether we strike a body, or the body strikes us; or, in other words, the giving away of force, and the receiving of force, produce the same sensation. If we hold one of our hands at rest, and strike the knuckles of that hand, with those of the other hand in motion, the knuckles of the hand which gives away the force, and those of the hand which receives it, will experience the same sensation. Thus it seems that the so-called resisting inertia of matter, is at times a mere *mental delusion*. I would particularly desire to impress it upon the young reader, that to *impart* motion to a body, means nothing more than to make to that inert, unresisting body a voluntary *gift* of a quantity of motion, taken as it were from our own stock, which of course becomes diminished to precisely the same extent; and moreover, that we *feel* our loss of the motion so *given* away. Also, that in order to *give* motion, or moving force to a body, we have only to place the force *in contact with* the body; or, as it were, to carry it to the body. When this much is done, the force will, by its own mysterious agency, *enter* the body, and *move* it.

We know that the Almighty has empowered man, within certain limits, not only to create moving force, by a mere exercise of will;

but to impart it to matter around him. I cannot think of a more satisfactory experiment for exhibiting this, than by means of a common spring balance. Hold in the hand a spring balance, from which say 1 lb. weight is suspended, at rest, by a long string out of a window. Here we have 1 lb. of force of gravity re-acting downwards upon the weight; and 1 lb. of our own force re-acting upwards upon it in the opposite direction. Consequently the weight remains at rest. Now we may *will* that there shall exist in the hand a considerable amount of upward moving force; and moreover that we will take that force out of the hand, and give it to the weight; so that the force shall cease to move the hand, and employ itself in moving the weight. To effect this, nothing more is necessary than to will to jerk the hand rapidly upwards. At that instant the index of the balance, and the rapid jump as it were upwards of the weight through a distance far greater than that through which the hand moved, will show that the weight has *accepted*, not *resisted*, a motion of several foot-pounds. Instead of 1 pound, the index will for a moment mark 2, 3, 5, or 10 pounds, according to the quantity of motion we give away. The hand will at the same moment experience a *real* resistance of 1 pound from the force of gravity acting on the weight; together with an *apparent* resistance equal to what was indicated by the index, minus the 1 lb. of gravity. This apparent resistance is owing to the loss of moving force, which the hand experienced at the moment of giving it away to the weight; and is precisely similar to what it would have experienced if it had actually met with a resistance by striking against a spring bumper above it. Were it not for the re-action of gravity, and the air, the 1 lb. weight would have continued to move upwards for ever, under the influence of the motion imparted by the hand in one instant.

Now can any reader imagine that there resided in the 1 lb. weight, any property called inertia, which could enable that 1 lb. of force to *resist* to the extent of 2, 5, or 10, &c., lbs., *just as it chose?* Or can he imagine that if the weight had offered any such *downward resistance*, it could notwithstanding have *moved upwards*, as it did? How is it possible to reconcile the two ideas? We all know that we can impart to a book lying on a table, a motion sufficient to make it move a foot or two; or one sufficient to send it flying across the room. In the first instance our hand *seems* to feel a slight resistance; and in the second, a great one. But it really experiences no other resistances than those arising from friction, and the air; both very trifling. What it chiefly feels, and mistakes for resistance, is the loss of that motion which it *gave away* to the unresisting book.

I conceive, therefore, that in imparting motion to matter, no other principle is called into exercise than extraneous moving force; that the matter itself takes no part in the operation; but being inert, yields passively: nor do I know of any tenable argument for introducing such a fancied interferer between moving force, and motion, as inertia. Moving force *is* motion. To say that motion in matter is the result of applied moving force, is merely saying that when we *put* motion into a body, that motion *is in* the body.

It also appears to me that in a scientific point of view, all the functions of force may be classed under two heads, *action* and *re-action*. The former regards force only in relation to *unresisting matter;* and its effect on matter is then velocity, only, without pressure; or matter *in motion*. The latter regards force only in relation to *resisting force;* and its effect on matter is then pressure, only, without velocity; or matter at *rest*.

Force when *acting* on matter (in my sense of the word) is called *dynamic*, or *moving* force, because it is then always in motion. When *re-acting* on force, it is called *static* force; that is, stationary or standing-still force: because it is then always at rest. *Strictly speaking*, force cannot *overcome* force; it can only re-act against it, and keep it *at rest;* or in equilibrium.

All the *motions* we see in machinery of every kind, whether driven by the force of either steam, water, wind, or animal power, are the result of the *action* of force on unresisting *matter*. Force, when applied to machinery, *divides itself* into two distinct portions, each for producing an entirely different effect from the other. One portion of it goes to work as re-action, or static force, to *re-act* against the static *forces* of friction, atmosphere, gravity, &c., which would obstruct motion. The other part, as *action*, or dynamic force, acts upon the inert matter composing the machinery. The matter accepts it unresistingly, and *is moved* by it, while the static force is holding in check the static force of friction, and other resistances to motion. The entire force remains all the time unchanged in its nature; it is the same identical force; but one part is engaged in one duty, and the other part in another duty; for static and dynamic force differ only in the kind of work they perform. Static force performs *rest;* dynamic force performs *motion*.

I have said that re-acting, or static forces, or forces *at rest* in consequence of their *re-action* against each other, produce *pressure* only, without velocity. I use the word pressure in acquiescence with one of those silly perversions of common language, which scientific writers seem so much to delight in. Static force produces *pulling*, as well as pressure, or pushing. Philosophers say that a pull and a push are the same thing; because they are both the result of static force; and that the word *pressure* covers both cases. If they desire a word that will really cover both cases, might they not adopt *re-action?* That word covers both cases, all such cases, and none but such cases: and could therefore lead the young student into no error: the *re-action* of extraneous forces *never* produces motion; but only rest, with pressure or pulling.

If I am correct in my opinion that velocity is the unresisted *action* of extraneous force on *matter*, then those writers (including, I believe, *all*) are in error who maintain that "*every action of force is attended by an equal, and contrary re-action.*" Those are the words in which the writer of the article "Mechanics," in the Encyclopædia Metropolitana, quotes the third one of Newton's three Laws of Motion; and

they are used in the same connexion by many writers. I admit that when force acts against force, action and re-action are always equal and contrary: but I contend that matter never re-acts against force, although force acts upon it; and I maintain that nothing but force re-acts against force. If matter re-acted against force, as writers affirm, there would be no such thing as motion, for the effect of re-action is rest. I would suggest that Newton's third Law should read thus:—"*Every action of force,* AGAINST FORCE, *is attended by an equal and contrary re-action.*"

If two men at the two ends of a rope pull against each other, each with a force of 20 lbs., the rope will be strained by a 20 lbs. re-action: or if they push against the two ends of a bar of wood in like manner, the bar will be strained by a 20 lbs. re-action. If one of the men should stop pulling, or pushing, the rope or bar ceases to be strained at all; it will merely accept the force of the other man, and be moved by it.

If a ball with a motion of 20 foot-pounds per second, encounter a similar ball moving with the same quantity of motion in the opposite direction, there will ensue a re-action of 20 lbs. of static force; for only static forces re-act.

If a ball with a motion of 20 foot-pounds per second, encounter a similar ball moving with but 1 foot-pound of motion in the opposite direction, there will ensue a re-action of 1 lb. of static force.

If a ball with a motion of 20 foot-pounds per second, encounter an equal ball *at rest*, there will ensue (according to my ideas) *no re-action* whatever. But the philosophers maintain that there will ensue 10 lbs. of re-action; or ten times more than in the preceding case; or that *no* re-acting force, re-acts more than *some* re-acting force.

What is re-action, but the opposition of force by equal force in the opposite direction? And in the last case (that of the ball at rest), where do we find any equal force in the opposite direction, that shall re-act against the moving force in the moving ball? Do the philosophers mean to assert that for want of such second force, the force in the moving ball will *re-act against itself;* that it will divide itself into two *antagonistic* forces, moving in opposite directions; and that these forces will re-act against each other, like two butting rams?

I conceive that the moving ball merely carries moving force *to* the ball at rest: that then this force by its own mysterious equalizing agency, *enters* the ball at rest *unresistedly;* and *co-acts* with the motion left in the moving ball, in giving equal motion to both balls. No motion has been lost during this process, because there was no second force to re-act; and consequently there has been no re-action.

I shall, therefore, venture to assert that matter *does not re-act* on force, nor force on matter; that force only *acts* on *matter;* and only *re-acts* against equal opposing force. That *force is always transferred, and received equally;* or in other words, that force transferred by one body, is always equally received by another body. To accept a

present, does not involve re-action; but to accept it, and at the same time to make another present in return, does involve re-action.

The young reader may not readily understand how transfer and acceptance can be equal, when two unequal forces meet in opposite directions. In such cases, as in that of all machinery just referred to, the greater force *divides itself* into two parts, one of which is precisely equal to the smaller force. This part alone *re-acts* against the smaller *force* to which it is equal, and brings it to rest; the two being converted, at the instant of contact, into static force. The only part of all the original two forces which continues to be dynamic, is the other portion of the larger force. This takes no part in the *re-action* operation, but persistently occupies itself in *acting* onwards. Thus we see that two unequal forces cannot re-act on each other; but that only the whole of the smaller one, and a *part* of the larger one, do so.

When we say that static forces keep each other *at rest*, we mean at rest *relatively to each other*. Two forces may thus be said to be at rest, although actually in rapid motion from the effect of some other force. Thus two men may pull or push against each other in a railroad car, exerting static force, so that neither can move the other. Relatively to each other, their forces are at rest, although the car may be moving at 50 miles per hour.

Most of the examples adduced by writers to prove the supposed re-action of matter against force, are quite puerile. A common one is that of a horse towing a canal boat; in which case we are told that the boat pulls the horse, as much as the horse pulls the boat. I conceive, however, that it requires no very scientific acumen, even in the young reader, to perceive after what has been said, that the boat, being inert matter, cannot re-act against the horse; but that his force is partly occupied in re-acting against the resistance of the *water* between him and the boat, (especially when towing against a freshet;) and partly in moving himself forward. The boat being fastened to him, follows passively, after he has once imparted motion to it; and if it were not for the resistances of the water and air, would follow him till doomsday, if he would only keep straight on: and moreover, it would not require any further force from him than what he imparted during his first few steps forward. But the resisting force of the water is constant, and the horse is compelled constantly to re-act against it. At first starting, he not only had thus to re-act against the force of the water, but at the same instant to give away a part of his moving force to the boat, to enable it to follow him. He feels the loss; but as he afterwards has only the resistance of the water to encounter, he gets along with more ease.

A railway train is standing on a perfectly level track; and force is required to put it into motion. Steam is let on; but the rails are covered with sleet, and are so slippery that, although the driving wheels fly around with great rapidity, the train does not budge. What is the cause? According to my idea, the train itself being composed of matter, is inert, and cannot re-act, or resist the force of the engine in the

slightest degree. There is no wind blowing, so that the atmospheric resistance at starting is about 0; and it has recently become the fashion among scientific men, (~~Morin leading off;~~) to say that *where there is no motion, there is no friction;* therefore there is no resistance from friction in this case. The engine could run away easily by itself, but it cannot move with the train. The scientific passengers ascribe the chief resistance to *inertia*, of course; because *it* will account for everything relating to force or motion, which cannot be accounted for in any other way. A marvellously convenient principle is that inertia, for, as we saw in the example of the spring-balance, *it can increase itself at pleasure;* and in the case now before us, the inertia of the train seems, as Newton has it, "*to endeavor as much as it can*," to keep us back.

I do not myself agree with the no-motion, no-friction gentlemen; and feel so strongly persuaded that our delay is caused by the friction of the train alone, that I suggest to the engineman, on the principle of "similia similibus curantur," to give his driving wheels a little more friction on the rails, to re-act against that of the car wheels. He does so, by sanding the rails; then he lets on steam again; the driving wheels again revolve; and sure enough the train starts. Now at least there is friction; and now, what duty has the engine to perform? Why the time-table requires that it shall take the train to the next station, at a velocity of 30 miles per hour. To do this, it must unceasingly furnish sufficient static force to re-act against the unceasingly re-acting forces of friction, and of the atmosphere; and must besides put into the matter composing the train, and its load, sufficient moving force to produce a velocity of 30 miles an hour. Now the engine has not force enough to do all this at one effort; but is compelled to manufacture it, and store it up in the train by degrees. When the engine shall have gradually made, and have transferred from itself to the train, a sufficient amount of moving force to produce in the train a velocity of 30 miles an hour, then this force will remain in the cars, and will continue to move them all the way to the next station, without any further help from the engine; and the engine will thenceforth have nothing to do, except to continue the manufacture, and imparting of re-acting force against the re-acting forces of friction and the atmosphere. When the train gets near the next station, the engine and brakes will even be compelled to take this *acting*, or *moving* force, out of it; otherwise it would not stop. They do this by creating more *re-acting force* of friction, which requires the moving force of the train to become static, in order to *re-act* against it, and thus produce *rest* in the matter of the train.

The driving wheels make their first revolution. The force imparted during that operation would have started off the engine alone at a great velocity; but as it diffused itself through the whole train, it could only move so large a mass at a slow velocity. This first revolution then has sufficed to impart sufficient force to re-act against friction, and the air; and to leave a small surplus, which as dynamic force, spreads itself through the matter of the train, and gives it a velocity

of say one-quarter of a mile per hour. Thus we see that it co-acts with the engine, which in the meantime (having given away to the train the force made during the first revolution), is engaged in making another revolution, and thus manufacturing a second instalment of force. All the surplus of this second instalment, beyond what is necessary for balancing the re-acting friction and air, also diffuses itself among the matter of the train; and assists the first instalment in giving velocity thereto.

The two instalments, by their combined forces, are now moving the train at half a mile an hour; thus leaving the engine to manufacture only 29½ miles per hour of moving force. The reader will observe that there is no *re-action*, or *resistance* to the force of the engine, caused by these two instalments of *moving* force; but on the contrary, hearty *co-action*. If they had re-acted, or had met with any re-action of inertia from the matter of the train, as the philosophers say they must, it is plain that instead of moving the train, they would have come to rest; and that the engine might continue for ever paying in small instalments of moving force, which would be re-acted against, and brought to rest one by one, as fast as the engine handed them over to the matter of the train. But, I maintain, that matter does not *re-act* against moving force; but *accepts* all we *give* it, and *moves* with a velocity precisely in proportion to it; and in the same direction.

So revolution after revolution of the drivers is made, each with greater rapidity than the former, in consequence of the united *co-action* of all the previous instalments; until at last the engine has taken from itself, and has put into the train, enough moving force to give it a velocity of 30 miles per hour. The road being level, the engine no longer takes any part in moving the train; and has nothing to do but manufacture sufficient force to *re-act* against the constantly re-acting *forces* of friction, and the air. The moving force which the engine has put into the train, would continue in it, and maintain motion in it forever, without any engine, were it not for these re-acting forces. In practice the re-action of gravity on grades, would also have to be re-acted against; but to simplify the example, the consideration of this, and of some other particulars, has been omitted.

A learned professor who happened to be in the cars, with one of his students, availed himself of the opportunity to explain that the slow motion at starting was owing to the tremendous resistance with which the inertia of the train re-acted against the engine; almost defying its utmost power. The student asked whether this tremendous force always jumped off, when the cars got to 30 miles an hour; but the professor explained that it did not; but that overcome by its own frantic efforts, it became exhausted; and would not recover until the next stop. An old lady, who had overheard the explanation, expressed herself greatly relieved thereby. For her part, she thought busting the biler was bad enough, without having them other rampaging things getting into the cars, and trying "as much as they can," to pull the ingine back.

(To be Continued.)

Preliminary Note on the production of Vibrations and Musical Sounds by Electrolysis. By GEORGE GORE, Esq.

From the Lond. Mechanics' Magazine, April, 1862.

If a large quantity of electricity is made to pass through a suitable good conducting electrolyte into a small surface of pure mercury, and especially if the mercurial surface is in the form of a narrow strip about $\frac{1}{8}$th of an inch wide, strong vibrations occur; and symmetrical crispations of singular beauty, accompanied by definite sounds, are produced at the mutual surfaces of the liquid metal and electrolyte.

In my experiments the crispations and sounds were readily produced by taking a circular pool of mercury from 1 to 3 inches in diameter, surrounded by a ring of mercury about $\frac{1}{8}$th or $\frac{1}{16}$th of an inch wide, both being contained in a circular vessel of glass or gutta-percha, covering the liquid metal to a depth of about $\frac{1}{2}$ an inch with a rather strong aqueous solution of cyanide of potassium, connecting the pool of mercury by a platinum wire with the positive pole of a battery capable of forcing a rather large quantity of electricity through the liquid, and connecting the ring of mercury with the negative platinum wire. The ring of mercury immediately became covered with crispations or elevated sharp ridges about $\frac{1}{16}$th of an inch asunder, all radiating towards the centre of the vessel, and a definite or musical sound was produced capable of being heard, on some occasions, at a distance of about 40 or 50 feet. The vibrations and sounds ceased after a short time, but were always reproduced by reversing the direction of the electric current for a short time, and then restoring it to its original direction. The loudness of the sound depends greatly upon the power of the battery; if the battery was too strong the sounds did not occur. The battery I have used consists of 10 pairs of Smee's elements, each silver plate containing about 90 sq. inches of immersed or acting surface; and I have used with equal success six Grove's batteries, arranged either as 2 or 3 pairs, each platinum plate being 6 inches long and 4 inches wide. If the cyanide solution was too strong, the sounds were altogether prevented.

Being occupied in investigating the conditions and relations of this phenomenon, with the intention of submitting a complete account of the results to the notice of the Royal Society, I refrain from stating further particulars on the present occasion.

Proc. Royal Society, April 11, 1861.

For the Journal of the Franklin Institute.

Strength of Cast Iron and Timber Pillars: A series of Tables showing the Breaking Weight of Cast Iron, Dantzic Oak, and Red Deal Pillars. By WM. BRYSON, Civ. Eng.

(Continued from page 48.)

An account of the "Fall of the Pemberton Mill" was published in the *Journal of the Franklin Institute*, vol. xxxix, page 242, and an article by O. B. M., entitled "A Practical Inquiry into the Cause of the Fall of the Pemberton Mill," was published in the *Scientific*

American of March 10, 1860, page 162. As I proceed, I purpose giving extracts from these and other papers.

Extracts from the *Scientific American*:—

"The pillars in the lower story were 6 inches in diameter at bottom, and 5 inches near the top, and designed to be $\frac{5}{8}$ inch thick; the middle or breaking part $5\frac{1}{2}$ inches in diameter."

"I measured several pillars that were broken by the fall of the building near the middle, and found the above pillars $\frac{3}{10}$ths to $\frac{3}{8}$ths of an inch thick on one side, and $1\frac{1}{8}$ to $\frac{7}{8}$ths of an inch thick on the other; and their length $12\frac{1}{2}$ feet. On the top of this pillar, and cast separate from it, rested a plate 12 inches in diameter, and $1\frac{1}{2}$ inches thick; on this plate rested the timbers with a hole cut out for the pintle, which was 3 inches in diameter, and extended up $16\frac{3}{4}$ inches to a flanch 7 inches in diameter and $1\frac{1}{8}$ inches thick, cast with the pintle; on this flanch rested the column above."

"Since the disaster, I have examined the thickness of several pillars in the mills at Lawrence, and find but little variation; the greatest not exceeding $\frac{3}{10}$ths of an inch."

"Many pillars of the Pemberton Mill were broken a little above the middle of their length—about $\frac{6}{10}$ths to $\frac{7}{10}$ths of their length, measuring from the bottom. Take one, for instance, that measured $5\frac{3}{8}$ inches in diameter at the bottom, $4\frac{1}{2}$ at small part near the top, and $11\frac{1}{2}$ feet long; it broke $7\frac{1}{2}$ feet from the large end, where the diameter was $4\frac{3}{4}$ inches; thickness at place of fracture, $\frac{3}{8}$ths of an inch on one side, and $\frac{1}{10}\frac{3}{2}$ths on the other. This adds new evidence, that when we make pillars tapering, we sacrifice strength to imaginary beauty."

Extract from the *Journal of the Franklin Institute*, page 247:—

JAMES B. FRANCIS testified—"By a column six inches in diameter, I should understand that this was the diameter at the middle. A column five and three-quarter inches in diameter, five-eighths inch thick, and twelve feet long (which is said to be the size of the columns in the lower story of the Pemberton Mill), according to the rule given by Hodgkinson, has a breaking weight of two hundred and thirty-one tons. A column, five and a half inches in diameter, five-eighths inch thick, and twelve feet long, by the same rule, has a breaking weight of two hundred and three tons. A column, five and three-eighths inches in diameter, five-eighths inch thick, and twelve feet long, has, by the same rule, a breaking weight of one hundred and ninety tons."

Reducing the above calculations for the breaking weight, which are for tons of 2000 lbs., to tons of 2240 lbs., they correspond with the value of w in my calculations, as shown in the following table by the first formula, and become respectively thus, viz:—

Ton of 2000 lbs.		Ton of 2240 lbs.
231 tons	=	206·25 tons.
203 "	=	181·25 "
190 "	=	169·64 "

But then this value of W cannot, according to Mr. Hodgkinson, be taken as correct for the breaking weight, the height of the pillars all being less than 30 times their diameter, as shown in the following table.

Hollow Uniform Cylindrical Pillars of Cast Iron, Both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	External diameter in inches.	Internal diameter in inches.	Number of diameters contained in the length or height.	Value of W in tons from formula $W = 44{\cdot}3\,\frac{D^{3{\cdot}6} - d^{3{\cdot}6}}{L^{1{\cdot}7}}$	Value of c.	Calculated breaking weight in tons from formula, $Y = \frac{W\,c}{W + \frac{4}{9}c}$.	Calculated breaking weight per sq. inch of section in tons.
12	5·75	4·5	25·04	206·38	493·08	176·61	17·55
12	5·5	4·25	26·18	181·41	469·02	159·58	16·67
12	5·375	4·125	26·79	169·66	457·00	151·31	16·22
				$W = 44{\cdot}34\,\frac{D^{3{\cdot}55} - d^{3{\cdot}55}}{L^{1{\cdot}7}}$			
12	5·75	4·5	25·04	187·62	493·08	165·96	16·49
12	5·5	4·25	26·18	165·32	469·02	149·95	15·66
12	5·375	4·125	26·79	154·81	457·00	142·19	15·24
				$W = 42{\cdot}347\,\frac{D^{3{\cdot}5} - d^{3{\cdot}5}}{L^{1{\cdot}63}}$.			
12	5·75	4·5	25·04	193·63	493·08	169·45	16·84
12	5·5	4·25	26·18	171·04	469·02	153·44	16·03
12	5·375	4·125	26·79	160·38	457·00	145·67	15·61
				$W = 40{\cdot}65\,\frac{D^{3{\cdot}55} - d^{3{\cdot}55}}{L^{1{\cdot}7}}$.			
12	5·75	4·5	25·04	197·39	493·08	171·59	17·05
12	5·5	4·25	26·18	173·93	469·02	155·18	16·21
12	5·375	4·125	26·79	162·88	457·00	147·21	15·78

Hollow Uniform Cylindrical Pillars of Cast Iron, of the same dimensions as those in the above tables, Both Ends being Rounded or Irregularly Fixed.

Length or height of Pillar in feet.	External diameter in inches.	Internal diameter in inches.	Number of diameters contained in the length or height.	Calculated breaking weight in tons from formula, $W = 13\,\frac{D^{3{\cdot}76} - d^{3{\cdot}76}}{L^{1{\cdot}7}}$.	Calculated breaking weight per square inch of section in tons.
12	5·75	4·5	25·04	82·29	8·17
12	5·5	4·25	26·18	71·77	7·49
12	5·375	4·125	26·79	66·85	7·16
				$W = 13\,\frac{D^{3{\cdot}6} - d^{3{\cdot}6}}{L^{1{\cdot}7}}$.	
12	5·75	4·5	25·04	60·56	6·01
12	5·5	4·25	26·18	53·23	5·56
12	5·375	4·125	26·79	49·78	5·33

(To be Continued.)

On some of the Causes, Effects, and Military Applications of Explosions: An Abstract from a Lecture by F. A. Abel, Esq., Director of the Chemical Establishment of the War Department.

From Newton's London Journal, May, 1862.

A glance was taken at the general nature and causes of the phenomena termed explosions, and attention was then specially directed to those explosions which are due to chemical agency.

In all instances of chemical action accompanied by an explosion, the production and violence of the latter are either entirely or principally due to the sudden and very considerable development of heat, which results from the disappearance for the time of chemical activity. The violence of such explosions is, therefore, regulated by the energy of the chemical action, or the degree of rapidity with which the chemical change takes place. There are instances in which the change of state, *e. g.*, the conversion of solids into vapors and gases, resulting from chemical action, and the suddenness with which this transformation occurs, would suffice to produce explosive effects, quite independently of the effects of heat developed by the change; but in all such instances, the sudden increase in volume of the matter, resulting simply from the chemical change, is insignificant as compared with the expansive effect exerted at the same time by the heat developed in consequence of the sudden and violent disturbance of chemical equilibrium. Thus, the actual volume of gas produced on the decomposition of gunpowder, though very considerable in comparison with that of the original solid, is but small when compared with the volume which it occupies at the moment of its production when under the influence of the intense heat resulting from the chemical change.

Explosions are occasionally produced by energetic chemical combination between elementary substances. Thus, potassium combines with bromine with explosive violence, in consequence of the powerfully expansive effect of the heat resulting from the intense and sudden chemical action between the two elements. Again, the union of hydrogen with oxygen or chlorine is so energetic, that the resulting water or hydrochloric acid is suddenly and enormously expanded by the heat developed—a powerfully-explosive effect being consequently produced.

Explosions are much more frequently the result of chemical decomposition. Several classes of compounds are known, the unstable character of which endows them with explosive properties. Thus, the compounds known as the chloride, iodide, and bromide of nitrogen, are highly susceptible of instantaneous decomposition; the very slightest disturbing causes sufficing to destroy the chemical equilibrium which exists between their component particles. Compounds of silver and gold with nitrogen, hydrogen, and oxygen (fulminating silver and gold), and of silver and mercury with a peculiar organic group, generally known as fulminic acid (the fulminates of mercury and silver), are also highly susceptible of sudden, and therefore violently explosive, decomposition. By the action of nitric and nitrous acids

upon several organic bodies, compounds of highly explosive characters are produced, their formation resulting from the abstraction (by oxidation) of a proportion of hydrogen-atoms from the original body, and the introduction, in their place, of a high oxide of nitrogen. The products of the action of nitric acid upon starch and cotton, in different forms, are the best known of these; among others, the substances known as nitromannite (obtained by the action of nitric acid upon mannite) and nitroglycerine, or glonoïne (the product of the action of nitric acid at low temperature upon glycerine), are remarkable for the violence with which they explode when submitted to friction or concussion.

One of the most recently discovered and curious of these explosive organic bodies, is the nitrate of diazobenzol, obtained by the action of nitrous acid at a low temperature upon aniline. This substance explodes at least as violently as iodide of nitrogen and fulminate of silver, if exposed to a heat approaching that of boiling water; it is, however, far less sensitive to friction than those two bodies. Similarly explosive substances have been quite recently obtained by Dr. Hoffmann from derivatives of the interesting and important base, rosaniline, the salts of which furnish some of the most beautiful of the colors now obtained from aniline.

Explosions are most readily produced by establishing chemical action between certain substances greatly opposed to each other in their properties, and brought together in an intimate state of mixture. The substances applicable to the production of such mixtures are, on the one hand, bodies remarkable for their great affinity for oxygen; and, on the other, compounds containing that element in abundance, and partly or entirely in a loose state of combination. To the first class belong the elements carbon, sulphur, and phosphorus, and compounds of the last two with readily oxidizable metals. The second class includes a few of the higher metallic oxides (such as the higher oxides of manganese and lead) and combinations of metals with nitric, chloric, and perchloric acids. Mixtures produced with these two classes of bodies readily ignite, or afford explosions, either upon the direct application of heat, or by submitting them to friction, percussion, or concussion; and, in a few instances, by establishing chemical action in a small portion of the mixture, with the aid of some other compound. These explosive mixtures vary greatly in the ease with which chemical action is established in them, and in the rapidity and violence of their transformation; their properties are naturally regulated by the chemical and physical characters of their constituents, and by the degree of intimacy of their mixture.

The variation in their explosive properties, and the great extent to which the characters of any particular mixture may be modified, are very important elements in their application to practical purposes; while the comparatively instantaneous nature of the decomposition of explosive compounds, and the facility with which it is brought about, present very great, and in many cases insuperable obstacles to their employment as explosive agents. By the comparatively gradual de-

composition of an explosive mixture, such as gunpowder (when employed as a charge in a gun), the force exerted by the gases generated in the confined space, discovers, before it attains its maximum, that portion of the chamber enclosing the powder (*i. e.* the projectile) which is separated from the remainder. By the motion which it immediately imparts to this, the *smaller* mass, the strain upon the *larger* mass, forming all but one side of the chamber (*i. e.* the breech of the gun), is at once relieved, while the force continues to the close of its development to act in the direction of the mass which has once yielded to its influence, and thus propels the projectile. The explosion of a charge of fulminate, on the other hand, in the chamber of a gun, is so instantaneous that the maximum of force is at once developed, and the strain thus exerted within the chamber, at the same time that it overcomes the inertia of the projectile (or the movable side of the chamber), will also overwhelm the cohesive force which maintains the mass of the chamber entire, and the breech of the gun will therefore be shattered. Enclosed in a shell, a charge of a fulminate will produce a much greater shattering effect than gunpowder upon the metal enveloped, reducing it to a much larger number of fragments; but the pieces of the shell produced by employing gunpowder as the bursting agent, will be propelled with much greater violence, because there is still a development of force after the rupture of the shell; while, with the fulminate, the entire force is at once expended upon the bursting of the shell.

The very great extent to which the rapidity of explosion of gunpowder may be modified to suit different applications, is one of the most important properties possessed by this material. A very rapidly burning powder is necessary in many instances; for example, in shrapnel shells, in which the charge of powder is required to break open the shell without interfering, by any great dispersive effect, with the flight of the enclosed bullets or fragments of metal. In mortars, and short guns also, a quickly burning powder is required, as they afford a comparatively limited space for the combustion of the charge. If a slowly burning powder be employed in such arms, a portion of the unexploded charge is expelled together with the projectile,—the period between the first ignition of the powder and the expulsion of the shot or shell from the gun, being insufficient for the combustion of the entire charge. In long guns and in rifled cannon it is very important on the other hand, that the ignition of the charge of powder should take place gradually, so that the pressure exerted thereby upon the gun and the projectile should, after the first ignition, be as far as possible uniformly continuous during the passage of the shot or shell along the principal portion if not the entire length of the gun's bore. With the gunpowder which has been until quite recently in general use for large cannon, the actual explosion of a charge is almost entirely accomplished before the projectile has passed beyond the trunnions of the gun. Hence the rear portion of the weapon is subject to a strain which is enormous as compared to that sustained by the front part of the cannon. Numerous important advantages naturally

result from a more uniform distribution of the pressure over the interior of the gun; for instance, the necessity of constructing the part reaching from the breech to the trunnions of very much greater strength than the remainder (a measure which, in the production of cast iron cannon, involves considerable difficulties) is greatly diminished, and the risk of fracture of guns, or of their serious injury from submission to excessive strain, is considerably lessened. The explosive action of gunpowder may, it need hardly be observed, be easily regulated by the introduction of modifications in the proportions of the carbon, sulphur, and saltpetre employed in its manufacture, and in the degree of intimacy with which the ingredients are mixed. Both of these expedients interfere, however, with the extent of force ultimately exerted by a given weight of the gunpowder; since, in either case, the chemical action between the ingredients would be modified. The rapidity of combustion of gunpowder may, however, be admirably regulated, without introducing any alteration in its composition or in the perfection of its manufacture, simply by increasing or diminishing the size of the particles or grains constituting a charge; and also by modifying the degree of compression to which the gunpowder is subjected before or at the time of its conversion into grains or pellets.

By combining the application of uniform and accurately regulated pressure with modifications in the composition of gunpowder, and by thoroughly confining the material within a case or receptacle, so that, if ignited, it can only burn in one direction, admirable and valuable arrangements (known as fuzes and time-fuzes) are obtained for igniting charges of gunpowder in shells at any period during their flight, which may have been determined upon previous to the loading of the gun. By simple mechanical arrangements, regulating the amount of the compressed gunpowder which shall burn before the flame reaches the charge in the shell, the time of explosion is readily adjusted with the greatest nicety (subject, however, to variations depending upon the degree of density of the atmosphere, as recently shown by Dr. Frankland's researches). The principle of regulated compression, and of combustion in one direction, is applied to the preparation of rockets, signals, and numerous pyrotechnic arrangements,—other explosive mixtures being, in some instances, substituted for the gunpowder.

The advantages offered by materials of a much more powerfully or rapidly explosive character than gunpowder, when employed simply as destructive agents (for instance, in many classes of mining operations), have led to repeated attempts at the application, as substitutes for gunpowder, of highly explosive mixtures, readily obtainable in large quantities, in which chlorate of potassa is employed in the place of a nitrate, in conjuction with very oxidizable materials, such as the sulphides of arsenic and antimony, and compounds containing carbon and hydrogen (Callow's mining powder and white or German gunpowder are examples of such compounds). All attempts to manufacture and employ such mixtures have, however, invariably terminated in

more or less disastrous results, in consequence of the comparatively low temperature at which chlorate of potassa exerts its oxidizing power. Very slight friction or percussion suffices to inflame many of these mixtures, and the violence of their explosive action is, in many instances, as difficult to control as that of explosive chemical compounds. Even in the manufacture and employment of comparatively so safe an agent as gunpowder, which may be subjected without ignition to tolerably powerful friction or percussion, and to the direct application of any temperature below that which suffices to ignite sulphur (about 550° Fahr.), the neglect of strict precautions for excluding the possibility of a particle of the powder being subjected to sudden and powerful friction, may, and frequently does, lead to accidental explosions. The occasional accidents in gunpowder manufactories are generally enveloped in mystery, in consequence of their fearfully destructive effects; in all cases, however, where it has been possible to trace the causes of such explosions, they have been found in the wilful or accidental neglect of simple precautionary measures, indispensable to the positive safety of the works and operators.

The more highly explosive mixtures, and some few explosive compounds, though inapplicable as substitutes for gunpowder on account of their great sensitiveness to the effects of heat, have, in consequence of this very quality, received important applications in numerous ingenious contrivances for effecting the ignition of gunpowder. Well-known instances of such applications are:—The employment of fulminate of mercury in percussion caps; of a mixture of chlorate of potassa and sulphide of antimony, in arrangements for firing cannon by percussion and by friction, and for exploding shells by percussion or concussion; and of the same mixture exploded at will by being brought into contact with a drop of strong sulphuric acid, for the ignition of submarine mines or of signals.

Other mixtures combining a high degree of explosiveness with power of conducting electricity, have been successfully applied to the simultaneous ignition of numerous charges of gunpowder by electricity of high tension: by means of one of them, recently discovered, many mines may be simultaneously discharged, even by the employment of small magneto-electric machines; the necessity for the employment of voltaic arrangements in mining operations being thus entirely dispensed with.

One of the most highly explosive mixtures at present known, consisting of chlorate of potassa and amorphous phosphorus, has been most ingeniously applied by Sir William Armstrong to the ignition of his time-fuzes, and to the production of concussion and percussion fuzes, remarkable for the great ease with which they are exploded. The above mixture may be ignited by the application of a gentle heat, or by submission to moderate pressure; if it is made up into a hard mass by mixture with a little shellac varnish, the friction resulting from the rapid insertion of a pin's point into the material suffices to ignite it, even when it is well covered with varnish. Thus, in Armstrong's time-fuze, which, when fixed in its place in the head of the

shell, cannot, like ordinary fuzes employed in smooth-bore guns, be ignited by the flame of the exploding charge of powder (as the shell accurately fits the bore of the gun), the fuze composition is inflamed, immediately upon the firing of the gun, in the following manner:—A small quantity of the phosphorus mixture is deposited at the bottom of a cylindrical cavity in the centre of the fuze, and over it is fixed a small plug of metal, with a pin's point projecting from its lower end. This plug is held in its place by a pin of soft metal, which, by reason of the *vis inertiæ* of the plug, is broken when the gun is fired, and the pin then instantly pierces the pellet of detonating mixture, which, by its ignition, sets into action the time-fuze. The distance between the pin's point and the phosphorus mixture, before the explosion, is only one-tenth of an inch. This arrangement exemplifies in a striking manner the delicacy of action which may be obtained by a judicious combination of simple mechanical arrangements and highly explosive materials.

The variety of work accomplished by the explosion of a charge of powder in an Armstrong gun loaded with a shell—no less than five distinct and important operations being thereby effected before the shell leaves the gun—affords a most interesting illustration of the progress made in the application of explosives, and of the comparatively great control which may be exercised over the operations of those destructive agents.

Mica and its Uses.

From the Lond. Mining Journal, No. 1393.

Within the past few years, we have received several inquiries as to the commercial value of mica, the complaint being that the market appears to be very limited, and that few seem to know the purposes to which it is applied. In last week's Journal, an address was given, by our Canadian correspondent, of a dealer in the material; and we have since ascertained that Messrs. Nash and Liénard of Lime street, are almost the only wholesale dealers in London. Hitherto its use has no doubt been limited, the manufacture of ornamental letters for affixing on glass, and of shields for the collection of the smoke from lamp chimneys, being, probably, the applications which have consumed the largest quantity, though it has also been used to some extent for photographic and other purposes. Efforts were made to introduce it for safety-lamps, in the hope that greater light would be obtained with less danger than when the ordinary wire-gauze is employed; but all attempts in this direction have hitherto failed, and, so far as we can see, are likely to fail, for various reasons.

Recently, however, a new field has been in process of opening up, by the endeavor to apply mica, previously colored or metallized, to the decoration of churches, rooms, shops, frames, and other ornamental and useful purposes. The mica, from its unalterable nature, preserves the gilding, silvering, or coloring from deterioration, and from its diaphaneity, the articles so treated will preserve all their brilliancy.

They are further preserved in a state of perfect cleanliness from the action of all smoke, dust, or from marks caused by insects, which may all be removed by washing; the surface being always smooth and impervious, moreover, is capable of resisting the action of corrosive acids, and the metal or colored surface appears with greater brilliancy.

In describing the invention, Mr. Murray of Paris, the patentee, states that he thus prepares and applies the mica. It is first cut to the desired thickness with an ivory or steel knife, and it is then coated with a thin layer of isinglass diluted in water, and the gold or other surface is applied, after which it is allowed to dry (any article, gilt, silvered, or colored by the known process, may be covered with a thin sheet of mica by gluing). He then takes a pattern of copper, with a design cut out on it, and places it on the reverse side of the mica, and with a small brush removes any superfluous parts, the required design thus remaining on the parts which have not been brushed. He then applies colors, either one or more times, as considered necessary, and afterwards coats the whole with a solution of liquid glue diluted in spirits of wine, which is applied for the purpose of rendering the mica pliable. When this is effected, the mica is applied to the frame or other object, which is coated with glue or other adhesive material, which is allowed to become comparatively dry. Mr. Murray then applies the mica with the design on it to the frame or other object, when it is allowed to dry for about three hours, after which all superfluous parts are removed, and the external surface of the mica made smooth. In order to make the junctions of several pieces of mica imperceptible, he first glues them together with Venetian glue, and then applies a hot iron to the parts where the mica is joined together, when the parts will be completely united.

The value of mica depends upon the size of the sheets, and their transparency; the clear ruby-tinged being the finest, and the cloudy grey the least valuable. With regard to the mica from British possessions, it appears that the sale of Canadian has been much damaged through the carelessness of those shipping it. The first parcel, of about ¼ ton, which Messrs. Nash and Liénard received was sold at 2*s*. 1*d*. per lb.; and the second, of about ¾ ton, realized 2*s*. Since this, the quality has not been kept up; the third parcel, of about 1 ton, required careful sorting after arriving in this country, to render it marketable at all, and then sold one-half at 2*s*., and the remainder at 7½*d*., the net amount cleared and transmitted to Canada being only £144, or about 1*s*. 1*d*. per lb. The same firm has since undertaken to import mica from Calcutta, and the quality is so much superior to that from Canada, that the latter is now saleable only at a very low price. The Calcutta mica is, indeed, about equal to that from Siberia, and is at present readily saleable at from 2*s*. 6*d*. to 4*s*. per lb., according to quality, and the quantity taken. Owing to varying quality the price of mica varies considerably: Canada mica will range from 3*d*. to 2*s*., and Calcutta from 6*d*. to 4*s*. per lb.

The idea which some of our correspondents entertain that mica and

talc are identical, or nearly so, is quite erroneous; they may be readily distinguished from each other in several ways. Both are flexible, but talc is not elastic, whilst mica is; then, again, talc is so soft that it will yield to the nail, whilst mica is nearly as hard as calc-spar. But the chemical test is the most satisfactory, as mica is composed of a silicate of potash, with a silicate of the sesqui-oxide of aluminium, whilst talc is a silicate of magnesia, and seldom contains any potash, though Klaproth found 2½ per cent. in it.

A Golden Bronze for Statues, &c.

From the London Builder, No. 1003.

The Paris correspondent of the *Birmingham Journal* describes a novelty in manufacture which, he states, has been accepted with great interest by the metallurgists of the Government. The discovery appears to have been purely accidental, and to have originated in what was at first regarded as a disastrous mistake, made by one of the workmen in the employment of Mr. Hullin, of the bronze powder factory at Chatelherault. The spilling of some chemical matter (which of course remains a secret) into copper and iron filings has produced two new metals or alloys, possessing every quality of gold and silver excepting weight. Both are inoxidable, and as rich and bright in color as the real metals, of which they are rather to be deemed the humble rivals than the mere imitators. The price is fixed at fifteen francs the kilogramme; and its cheapness has induced the experiment to be made public by the execution of a statue of Saint Anne, for the hermitage of Auray, in Brittany. The statue is intended to be of colossal proportions, and to adorn the roof of the grotto. A tea service of the metal, it is added, has been sent to London for the Exhibition. Ancient bronzes were ordinarily gilt.

Ornamental Burnt Wood-work.

From the Lond. Builder, No. 996.

A patent mode of producing ornamental wood-work by burning, first practised in Manchester, is now being carried on in London,—Wenlock-road, City-road,—and may be usefully introduced in many cases. The poker-burnt pictures of some time ago have shown what an agreeable color and what an enduring result can be obtained by charring the surface of wood. In the practical works which have grown out of that, designs in relief are engraved on the face of hollow iron cylinders; and these, being heated by a gas-pipe within, acted on by a second pipe conveying atmospheric air, are made to transfer these designs to the planed board which is passed in between them as they revolve. When the wood has gone through the charring operation, it is handed to a workman, who scrapes it down over the surface so as to bring out the lights, and produce the best effects. This being accomplished, the face is varnished or polished, and the result is an orna-

mental panel or moulding of considerable beauty (*if the design be good*), and of remarkable permanence. White woods, as sycamore or lime, are employed for the work. It is easy also, by this process, to give to the less expensive varieties of wood the characteristic of the more costly kinds. Rosewood and walnut are very well imitated; and upon these again any pattern can be impressed. The cost of woods imitated in this way is put at two-thirds the cost of good hand "graining." There is a wide field wherein this ornamental burnt wood process may be usefully employed. To insure success, however, the company must endeavor to obtain good designs, in a higher style of art than the majority of those they are now working with.

Safe Working Pressure of Boilers, and Hooping of Flues.

From the London Mechanics' Magazine, June, 1862.

Mr. L. E. Fletcher, the engineer of the Manchester Association for the prevention of steam boiler explosions, in his last monthly report says:—

For some time since I have been desirous of touching upon the point of Safe Working Pressures for boilers, since it not unfrequently happens that it is necessary to warn our members on account of excess.

The scale adopted by the Association as a general standard is as follows:—For shells of boilers 7 feet in diameter, made of $\frac{3}{8}$th plate, the safe working pressure is 50 lbs.; if of $\frac{7}{16}$th plate, 60 lbs.; and other dimensions in proportion. This allowance corresponds with the general practice of the manufacturing engineers of the district, is quite as high as the standard in other parts of the country, and considerably in excess of that permitted either in France, Holland, or Belgium, by their respective governments. It must, however, be distinctly understood that this standard should not be applied in every case, without any allowance being made for the attendant circumstances. It is only applicable in cases where the boiler is well made, both as regards materials and workmanship, and where the condition of the plates is good. It would be highly dangerous to apply it to boilers weakened by the wear and tear of years; while, on the other hand, a new and thoroughly well made boiler might for a time be allowed to work at a pressure slightly in excess of that given. But this could only be safe where everything is in first-rate condition.

It is a very common idea that the bursting pressure of a boiler is six times as high as that given above as its safe working pressure. This, however, I am persuaded, is a great mistake, and leads in many cases to undue confidence. I am confirmed in this conclusion by the constant examination of the rent plates in boilers that have exploded, where I find that, even where explosion results from thinning of the plates, rupture ensues long before they are reduced to one-sixth of their original thickness, and in one case I knew a well made and nearly new boiler, in first-rate condition, to explode, on account of only a comparatively slight increase of pressure, which had accidentally been

allowed through an error in the steam gauge. In this case, that at which the boiler actually burst did not exceed its ordinary working pressure by more than 50 per cent., the one being about 90 lbs., the other about 60 lbs. I believe that an application of any thing like six times the pressure given in the scale above would burst most of the boilers in Lancashire, and where it has been actually attempted by hydraulic pressure, the steam domes have been found to tear off long before the strain referred to has been attained. I cannot, therefore, think that shells of cylindrical boilers can be worked without risk at a higher pressure than that given in the preceding scale, unless under very exceptional circumstances.

With regard to the furnace tubes which are exposed to external pressure, I am glad to find that the practice is becoming increasingly general of strengthening them either with flanched seams or hoops, the hoops being made either of angle iron, T iron, or other approved form; and since it too frequently happens that flues are not made in the first instance truly cylindrical, on which their strength so much depends, and that other sources of weakness creep into the manufacture unawares, it is extremely desirable that no new boiler should be constructed with flues unstrengthened in the way just described, however slight the working pressure may be.

These hoops are frequently added to boilers after their first construction, and since some of our members have suffered inconvenience from the imperfect manner in which they have been fixed, I may state the method found by experience to be the best, which is as follows: The hoops, if made in two halves, may be passed in through the manhole, and can then be secured to the furnace tubes when in position. They should not, however, be brought in direct contact with the plates of the tubes, but should have ferrules of about an inch thick placed between the two, so as to leave a clear space all round through which the water can circulate. Where this space has been omitted, the plates have been found in some places to crack at the rivet holes, and in others to blister and buckle, in consequence of which many plates have had to be cut out and the hoops removed, from which the system of hooping has been in some cases unfairly condemned. Where, however, the ferrules have been introduced and the water space allowed, no injury has been found to arise to the plates even over the hottest part of the fire. The rivets uniting the hoops to the furnace tube should pass through these ferrules, and be spaced about 6 inches apart, while the two halves of the hoops should be connected together by butt strips riveted to their ends at the back. When hoops are applied as an after-clap in this way, angle iron is preferable to T iron, as the flanch, being narrower, is less liable to cause overheating of the plate. It may be necessary to vary the size of the angle iron in some cases, but generally speaking, one 3 inches in the flanch and half an inch in thickness will be found to answer every purpose. It is sometimes the practice to put two angle irons back to back. This is quite unnecessary, and a single one is all that is required. A drawing, to show the arrangement recommended, has been made for the assistance of the

members, and can be seen on application at the Offices of the Association.

Since writing the above, I have met with some additional cases, where considerable expense has been incurred by having to remove angle iron hoops from furnace tubes, in consequence of the injudicious mode in which they have been fixed, and would therefore impress upon our members the importance of attention to the above, if they wish to prevent the recurrence of disappointment in their own case.

Preserving Timber.

From the London Builder, No. 1010.

An experiment has been made at Cherbourg, in preserving wood for vessels from being affected with dry rot, by subjecting the timber to a slight carbonization with common inflammable coal-gas. The cost is only about ten cents per square yard of framing and planking, and the result is stated to be completely successful.

Notice of the Death of Professor Barlow, F. R. S.

From the London Practical Mechanics Journal, May, 1862.

Mathematical science has just sustained a severe loss in the death of Professor Barlow, than whom, perhaps, no one has contributed more to the literature of the applied sciences of our times. This distinguished man was born in Norwich in 1776, and he displayed his mathematical prowess and great energy of character at a very early age. In 1806 he was appointed one of the mathematical professors at the Royal Military Academy at Woolwich, which office he held until 1847. He was the author of numerous works, among which are his well known treatises on the "Theory of Numbers," the "Strength of Materials," his "Essay on Magnetism," &c. His discovery of the means of correcting the local attraction on the compasses of ships brought him into great notoriety, and he received the Copley medal, and was elected on the council of the Royal Society. The Board of Longitude conferred upon him the reward provided for useful nautical discoveries. The Emperor of Russia acknowledged the value of the invention, and presented him with the diploma of the Imperial Academy of Sciences at St. Petersburgh, and he was elected a corresponding member of the Institute of France and Royal Society of Brussels, besides other rewards and honors. In mechanical subjects he was associated with Mr. Telford in experiments for the Menai Bridge. He was then called in reference to the removal of old London Bridge. In 1836 he was appointed one of the Irish railway commissioners, with General Sir John Burgoyne and the late Mr. Drummond, and subsequently appointed on three other Royal commissions relating to railways in England. In 1847 he retired from the Royal Military Academy, and the Government awarded him his full income for the

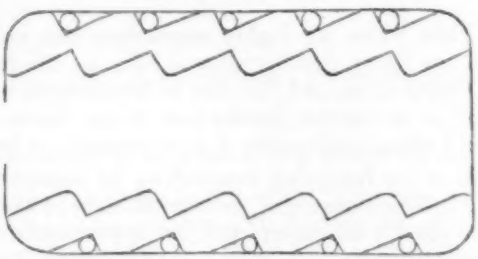
remainder of his life, in consideration of his eminent services. Enjoying the highest respect and admiration for his learning, and the way in which he used it during a long and active life, his memory well deserves some tribute from us now that he is dead.

Improved Omnibus.

From the Lond. Civ. Eng. and Arch. Journal, April, 1862.

An improvement in these vehicles has long been needed, but those constructed to give more sitting room and other conveniences have been of such increased size as to prevent their general adoption. Want of room—as most of our readers no doubt know by experience—is very often, to a great extent, occasioned by the immovable selfishness of one particular rider, who will secure ample sitting room for himself, regardless of the comfort of his fellow passengers. And where all are equally accommodating, it will be found that, while there is a great deal of squeezing and crowding about the shoulder region, the room on the seat itself is sufficient. Our attention has been called to a very ingenious arrangement recently registered, and which is shown in the accompanying diagram, exhibiting a plan of the seats by which

PLAN OF SEATS OF OMNIBUS.



these inconveniences are obviated. It will be seen at once that each person's seat is defined; without any division being used, while the passengers in sitting, as it were, overlap each other, thus securing perfect freedom and abundance of room to each. Our readers can try the effect of the improvement, by placing chairs at first side by side, and then obliquely against each other. The spaces behind the seats are for umbrellas—intolerable nuisances in an omnibus in wet weather. The increase of size is only eight inches in the length of the omnibus.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, July 17, 1862.

John C. Cresson, President, in the chair.

Isaac B. Garrigues, Recording Secretary.

The minutes of the last meeting were read and approved.

A Letter was read from l'Académie Royale des Sciences de Lisbonne.

Donations to the Library were received from the Royal Geographical Society, the Royal Astronomical Society, the Statistical Society, and the Society of Arts, London; l'Académie Royale des Sciences de Lisbonne, Portugal; M. Alexandre-P. Prevost Genéve, Switzerland; the Smithsonian Institution, and Frederick Emerick, Esq., Washington, D. C.; Dr. Charles H. Porter, Albany, N. York; Thomas C. Clark, Esq., Camden, N. J.; Hon. Charles Naylor, John C. Trautwine, Esq., John Warner, Esq., and the American Philosophical Society, Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's statement of the receipts and payments for the month of June was read.

The Board of Managers and Standing Committees reported their minutes.

The Board of Managers reported the following resolutions passed at their last stated meeting, held on the 9th instant:

Resolved, That the Board of Managers of the Franklin Institute have heard with great regret, the death of their fellow-member, Major John M. Gries, from the effects of a wound received in the service of his country.

Resolved, That while we highly appreciate the pure patriotism which induced our fellow-member thus to offer his life for the preservation of the Constitution and liberties of his country, we cannot but give expression to our sorrow for the loss of one whose services were so valuable, and whose association was so pleasant to us.

Resolved, That the foregoing resolutions be communicated to the family of our late fellow-manager, as a testimonial of the esteem which we entertained for his character, and the appreciation which we felt for his services.

Candidates for membership in the Institute (4) were proposed, and the candidate (2) proposed at the last meeting were duly elected.

The following preamble and resolutions offered by Frederick Fraley, Esq., were unanimously adopted:

Whereas, the members of the Franklin Institute have received information of the death of their late fellow-member, Major John M. Gries, an event which has suddenly broken a bond of usefulness and fraternity that has existed many years; and whereas, the merits and services of the deceased are held in grateful remembrance by the Institute, and the record of them should be appropriately perpetuated: Therefore

Resolved, That the Franklin Institute has heard with unfeigned sorrow the notice of the death of Major John M. Gries, in consequence of a wound received while in the service of his country.

Resolved, That the patriotism which led him into the field in defence of the noble institutions of the country, was of a kindred feeling to that which he manifested for many years in the service of the In-

stitute, and in the furtherance of the beneficent and useful purposes for which the Institute was founded.

Resolved, That feeling most sensibly the loss which the Institute has sustained by the removal of Major Gries, we deeply sympathize with his family and friends in their bereavement, and tender them our sincere condolence.

Resolved, That a copy of these preamble and resolutions be signed by the President and Secretaries, and be transmitted to the family of Major Gries.

Mr. Edward Brown exhibited a Heat Gauge or Pyrometer. This instrument is for indicating temperatures from 0 to 1000 degrees. It is constructed on the principle of the different expansion of two kinds of metal. A metallic rod about five feet long is inserted into a brass tube, and firmly fixed to it at the bottom end, the upper end is free, and is attached to a lever communicating by means of rack and pinion with a pointer. It has a clock face, and is in general appearance like a steam gauge. It is principally used for the hot air of blast furnaces, but may be also used for ascertaining the temperature of steam boiler flues, super-heated steam-wire annealing ovens, bakers' ovens, &c.

COMMITTEE ON SCIENCE AND THE ARTS.

Report on an Explosion of a Steam Boiler.

The Committee on Science and the Arts constituted by the Franklin Institute of the State of Pennsylvania, for the promotion of the Mechanic Arts, to whom was referred for examination—"An Explosion of a Steam Boiler at the Port Richmond Iron Works of Messrs. I. P. Morris & Co., Philadelphia, Penna.,"

REPORT:—That on the evening of Saturday, October 19th, 1861, about 15 or 20 minutes before 6 o'clock, and about 10 or 15 minutes after the fires had been drawn, the middle one of three boilers at the Port Richmond Iron Works of Messrs. I. P. Morris & Co. exploded. The rupture occurred partly through and partly close to a transverse seam of rivets near the middle of the length of the boiler, and at the part which was situated nearly over the bridge-wall of the furnace. The two pieces were projected horizontally with great force in opposite directions: One in its course passing through the back wall of the boiler house, the two walls of a small building, occupied by the foundry blowing machinery, which it nearly demolished, and through the brass foundry and its wall, and finally landing upon a heap of rubbish beyond. The head of this part was cracked across, but the plate-iron was entire. The other end struck a large cylindrical casting weighing about 20,000 lbs., breaking it into a number of pieces, and leveling a small brick outhouse, about 10 feet in diameter. These obstructions so changed the line of flight that the side of the boiler struck some of

the fragments of broken castings and of the outhouse, causing a portion of the boiler plate to become flattened and to tear. The cast iron head was broken into small pieces.

The boilers on each side were not injured, but were thrown sideways through the walls of the boiler house; one passing into the foundry and destroying some of the machinery, while the other threw down the wall of the coal depository. Each boiler had suspended beneath it a mud-drum, having three communicating pipes for the circulation of the water, and a common steam-drum; these were uninjured beyond the breaking of their attachments to the boilers. The sudden escape of the steam destroyed the roof of the boiler house, and a part of that over a wing of the foundry, and a quantity of bricks and broken timber were thrown upon the surrounding buildings.

The engineer and two assistants were on duty at the time. The engineer was found lying against the front wall of the fire-pit, in such a position as made it evident that he must have been killed instantaneously. One of his assistants (Hibbard) was at a hose-plug nearly in front of the boilers, and the explosion carried him several yards away among the fragments of the buildings, causing injuries which resulted in his death about six hours afterwards. The other assistant (Parker) was not injured bodily, but his nervous system was so shocked as to prevent him from giving a coherent description of the occurrence. Hibbard, before his death, made the following statement:—"The engineer after drawing the fires had made preparations for cooling the brick-work around the boilers, had attached the hose to a plug, and taken the branch-pipe into the fire-pit, directing me to open the plug. I told him not to play the water in there, but he disregarded my caution, and by his order I opened the plug." In corroboration of this statement, the hose with ¾-inch branch-pipe attached was found among the rubbish in the fire-pit, and one of the watchmen, who reached the place immediately after the accident, found the water turned on, and shut the plug.

An inspection of the scene next morning proved that the boiler must have had its supply of water, for the ground was coated with a stratum of mud about ¼ inch thick, formed from the water of the exploded boiler only—for the positions of the other boilers were such that the water escaping from them would have settled into excavations large enough to hold several times their contents.

The steam pressure was noted on the gauge in the engine room by the foreman of the machine shop, about half an hour before the fires were drawn, and was found to be less than the usual pressure.

An examination of the safety-valve of the exploded boiler, which was found attached to one portion of it, showed that it was of insufficient opening for the size of the boiler, and that its stem was entirely rigid and incapable of that amount of play which is indispensable to the free action of the valve; yet the time at which the explosion took place, so long after the fires had been drawn and the steam ceased to generate, seeming to render it impossible to account for this explosion

by the supposition of any undue pressure of steam caused by these defects of the valve.

The examination of the metal and construction of the exploded boiler showed no apparent defect in either. The two adjacent boilers were afterwards tested by the Committee under a pressure of 212 lbs., which they bore without any evidence of strain or approach to fracture.

If, however, the statement of the assistant (Hibbard) be believed, and it be assumed that the engineer was engaged in throwing a stream of cold water upon the bottom of the boiler, it appears to the Committee that the explanation of the explosion is easy and evident. The sudden cooling of the boiler over a limited area of its surface, would necessarily cause a change of its form and a sudden and immensely great strain in the direction of its transverse section, which might very probably result in a rupture, which if it took place would follow a line around the boiler, and at or near the point near the bridge wall, since this is the point at which the cold water would be thrown upon the metal. That the fracture should tend to follow the line of rivets which nearly corresponded with the line of greatest strain, is explained from the well known fact that such line is generally the weakest part of the boiler surface.

The Committee, therefore, believe that this supposition is the most probable in reference to the cause of the explosion, and strongly recommend the proprietors and managers of establishments using steam to forbid their engineers to throw streams of water upon the boilers while hot, as a practice fraught with danger, and destructive to the boiler even when it does not at once produce explosion.

By order of the Committee,

WM. HAMILTON, *Actuary.*

Philadelphia, July 10th, 1862.

BIBLIOGRAPHICAL NOTICE.

Report upon the Physics and Hydraulics of the Mississippi River; upon the Protection of the Alluvial Region against Overflow; and upon the Deepening of the Mouths: Based upon Surveys and Investigations made under the Acts of Congress directing the Topographical and Hydrographical Survey of the Delta of the Mississippi River, with such investigations as might lead to determine the most practicable plan for securing it from inundation, and the best mode of deepening the channels at the mouths of the river. By Capt. A. A. HUMPHREYS and Lieut. H. L. ABBOT, Corps of Topographical Engineers, U. S. A. Philadelphia, J. B. Lippincott & Co., 1861.

There is, perhaps, no subject of equal importance connected with civil engineering, of which so little is known, as the flow of water in large rivers. The extreme mobility of the water, causing it to yield

in any direction to the slightest forces; its cohesion, which seems so slight and negligable in experiments on a small scale, yet exerts a controlling influence upon the phenomena of movement of large masses; the irregularities of all kinds which affect the bed, every one of which produces changes which are propagated over extensive areas; the modifications of the motion, which are introduced by the affluent streams, the winds, and other meteorological changes, render the problem almost indefinitely intricate, and require for its solution experiments as numerous and varied as they are difficult and delicate. It is scarcely within the power of any individual engineer to approximate to its solution; for no set of observations, however numerous and varied, if confined to a single locality, would suffice to give the general law; nor, as it appears, are the formulæ for large and rapidly moving rivers to be deduced from observations on small or sluggish streams. The examinations of the great rivers of Europe, under the authority of the various governments, have commenced the solution of this great problem, but they have been too few, and the subject too vast to be completely solved even by their examinations; too many fundamental data have had to be assumed, and the means of verification have been too few and limited to give to their results the extreme accuracy which the ability and patience of the authors ought to insure. This, therefore, appears to be one of the physical problems which the United States Government seems especially designated to attack.

The Mississippi River from its length, its volume, the great variety of character which its bed presents in the different parts of its course, and the equally great change of character which its flow assumes at different periods of the year, seems to be especially fitted for such an investigation: the immense agricultural and commercial interests which concentrate on and around it, justify the outlay which it requires: the government has at its disposition a body of highly educated engineers, whose pride as well as scientific ardor would insure their devoting their best energies to the task: while the separate states through whose territories the river passes have found it necessary to employ for their own purposes high engineering talent, and thus have insured a series of checks upon the work which may not be without utility. These considerations explain why it is that in the report which we are now considering, Capt. Humphreys and Lieut. Abbot have made by far the most valuable contribution to the science of river-hydraulics which it has ever received, and have placed their names for the future in the same rank with those of the most distinguished of their predecessors. And this is true not merely in any one department of the investigation; the ability of the authors is equally shown in the skill with which the experiments were devised, the fullness and precision with which they were tried, and the extreme ingenuity and power with which they were discussed, and the resulting formulæ tested and verified. Future observers may, perhaps, find some things to correct, perhaps to add; but they have in the book before us a model of investigation which will form the basis of their own labors.

Nor must it be understood that the report is a mere dry scientific record of experiments and their discussion and results; the authors have found means to weave into their details a great mass of information which will be found interesting to every one who desires to learn; and while this work will be the subject of profound study by every hydraulic engineer, parts of it, we have no doubt, will be read with much interest by those who will not trouble themselves with the authors' mathematics.

We have not the room now to analyze and criticize this work as it deserves; perhaps hereafter we may take the opportunity to refer to it more fully, and to make such extracts from it as may be valuable to our readers. For the present we must confine ourselves to a brief account of its contents and method.

After a general account of the origin and progress of the work and a reference to other investigations bearing upon the subject, it proceeds to give a minute account of the physical geography of the basin of the Mississippi River, dividing this into subordinate basins, of which seven principal ones are described. In this chapter we have a great amount of very valuable information in reference to the soil and climate of the basins of the various great tributaries to the Mississippi, of their slopes and floods, which must be studied before their influence on the main basin can be understood.

The second chapter is devoted to a similar minute physical account of the river below the junction of the Missouri, its topography, and the geology of its bed; in describing which, the prevalent belief that the basin of the lower Mississippi was originally a gulf of the ocean which has been gradually silted up by the deposits of the river, through which its waters have cut their present bed, is shown to be an error; the slope, cross-section, and drainage; its temperature, and the amount of sediment which it causes; the history of the floods of the river so far as any reliable information can be obtained of them, and an account of the progress of the system of *levees* by which the plantations along its banks are protected.

The third chapter is devoted to an account of the various contributions which have been heretofore made to river-hydraulics; the theories being explained, the methods of observation described, and the resulting formulæ given.

Chapter IV. is devoted to a minute explanation of the method of gauging the river and its tributaries and crevasses used in the survey: including a very interesting and important discussion of the subject of the mean velocity of the stream.

Chapter V. discusses the new experimental laws of water in motion proposed by the authors, the formulæ by which these laws are expressed, and the tests to which these laws and formulæ were subjected, which it must be confessed were as numerous and as rigid as could be desired; and it is not a little gratifying to see how very satisfactory is the general accordance between the numerical values resulting from

the formulæ and those got by actual observation. It is true that before any new theory is adopted, a very protracted and careful study of it is necessary, and the confirmation of its results by other observers and under different circumstances from those in which it originated, yet it scarcely seems possible that such accordances as we here find with the severest tests, could be brought about by any thing but a conformity to the true natural laws.

Chapter VI. is devoted to the discussion of the subject of the protection of the land bordering on the river, from its floods, and the authors give what appear to be satisfactory reasons for relying in general upon the system of levees, which they show can be economically made effectual.

Chapter VII. is occupied with an account of the delta of the Mississippi, its extent, rate of increase, and probable age. The authors give four thousand four hundred years as the approximate age of the Mississippi since it assumed its present condition of a delta-forming river, and state their reasons for believing that it existed before this as a clear stream. They consider that the original mouth of the Mississippi was near the present efflux of *Bayou Plaquemine*, and that, consequently, its advance into the Gulf has been about 220 miles. The present rate of advance of the mouth is about 262 feet per annum, which will not probably be exceeded hereafter. An interesting explanation of the mode by which the branches of the main river become separated from it is also here given.

Chapter VIII. treats of the hydrography of the mouths of the river, the formation and profiles of the bars, and the proper methods for maintaining sufficiently deep water over them.

A large body of appendices, the first of which is the report on the the mouths of the Mississippi by Captain Talcott in 1838, and the rest are made up of the detailed observations, complete the work, which is illustrated by a number of valuable maps and diagrams.

In conclusion, we repeat what we have before said, that we have no hesitation in considering this report as the most valuable contribution which has yet been made in any country to the subject of river-hydraulics, and as indispensable to every engineer who is called upon to suggest improvements or execute works depending on this branch of science. F.

ERRATA.

In formula 6, page 54, the last term, b, should be negative, thus:

$$l = \text{sin. } v\,(h - r \text{ sin. } v) + r \text{ sec. } v - b.$$

The same correction should be made in example 2, formula 6, page 56.

METEOROLOGY.

For the Journal of the Franklin Institute.

The Meteorology of Philadelphia. By JAMES A. KIRKPATRICK, A.M.

JUNE.—The mean temperature of the month of June, 1862, was 4° below that of the same month, for the last eleven years.

The warmest day of the month was the 29th, of which the mean temperature was 79·8°. The maximum temperature for the month (89°) occurred on the same day.

The coldest day was the 8th, having a mean temperature of 58·2°. The minimum (47°) occurred on the 16th, of which the mean temperature was 58·3°, but one-tenth of a degree higher than the coldest day of the month.

The range of the temperature for the month was 42°.

The greatest change of temperature in the course of a day was 27°, on the 12th of the month; the least was 7°, on the 5th, when rain fell during the greater part of the day. The average daily oscillation of temperature (17·6°) was but a quarter of a degree more than the average for the month for eleven years, and about 1° more than for June, 1861.

The greatest mean daily range of temperature—that is, the greatest mean difference of temperature, between two successive days—was 15°, between the 14th and 15th; the least was between the 29th and 30th, when there was no difference whatever. The average daily range for the month (5·34°) was about the same as for June of last year, and about six-tenths of a degree greater than the average for the month for the last eleven years.

The atmospheric pressure was greatest (30·146 inches) on the 16th of the month, and least (29·375 inches) on the 30th. The mean pressure for a day was greatest (30·142 inches) on the 16th, and least (29·420 inches) on the 30th of the month. The average pressure for the month (29·724 inches) was three-hundredths of an inch less than in June, 1861, and seven-hundredths of an inch less than the average pressure for June for eleven years. It was less than in any June during the whole period of observation, except in the year 1857, when the average for the month was 29·663 inches.

The greatest mean daily range of pressure for the month was 0·38 of an inch, and occurred between the 17th and 18th days of the month; the least was 0·008 of an inch, between the 25th and 26th. The average mean daily range for the month (0·123 inch) was two-hundredths of an inch greater than usual.

The force of vapor, as will be seen by a reference to the table of comparisons, was about as much below that for June of last year, as the latter was below the general average. It was greatest (0·770 in.) on the 14th, and least (0·180 in.) on the 16th of the month. The average for the month (0·477 in.) was six-hundredths of an inch less than the average for June for eleven years.